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THE CLASSIFICATION OF ARTIFACTS IN ARCHAEOLOGY

IRVING ROUSE

ABSTRACT

Analytic classification consists of forming successive series of classes, referring to different features of artifacts. Each class is characterized by one or more attributes which indicate a custom to which the artisan conformed, for example, a technique of manufacture, or a concept which he expressed in the artifacts, such as a design. These customs and concepts constitute modes. They are "procedural modes" when they refer to behavior of artisans and "conceptual modes" when they consist of ideas which artisans have expressed in artifacts.

Taxonomic classification consists of formulating a single set of classes, one for each kind of artifact in the collection. Each class is characterized by two or more modes, selected from among the total number of modes obtainable by means of analytic classification. The modes diagnostic of each class constitute its type. If diagnostic modes are selected for their time-space significance, the resultant types are "historical." If the diagnostic modes are selected for what they indicate about the intrinsic nature of the artifacts, the types are "descriptive."

The situations under which it is best to use either procedural or conceptual modes or historical or descriptive types are discussed and it is concluded that all four kinds of units are essential for the complete interpretation of archaeological remains.

A NUMBER of recent papers, such as Phillips (1958), Wheat, Gifford, and Wasley (1958), and Sears (1960), have been concerned with particular methods used by archaeologists to classify artifacts. The present paper is instead an attempt to survey the range of current methods. It is intended to differentiate the various methods, to discuss their theoretical basis, and to assess their relative utility.

According to the dictionary (Nielson, Knott, and Earhart 1940: 496), the word classification refers to "the act of assigning [artifacts] to a proper class." If the class is a new one, it will have to be defined by listing the criteria used to form it and will also have to be given a name or a number. If pertinent classes had previously been established, it will be enough to determine that the new artifacts have the criteria diagnostic of one of the classes, and to give them the name of that class.

Classification, like statistics, is not an end in itself but a technique by means of which to attain specified objectives, and so it must be varied with the objective. The main opportunity for variation comes in selecting the criteria which are to be considered diagnostic of one's classes. In my experience, archaeologists select these criteria to meet one of two alternate objectives: either to form modes or to establish types. If modes are the objective, the classification is called "analytic" (Whiteford 1947). If, instead, the purpose is to form types, then the classification becomes "taxonomic" (Phillips 1958). I shall discuss these two kinds of classification in turn.

ANALYTIC CLASSIFICATION

By the term "mode" is meant any standard, concept, or custom which governs the behavior of the artisans of a community, which they hand down from generation to generation, and which may spread from community to community over considerable distances (Rouse 1939). Such modes will be reflected in the artifacts as attributes which conform to a community's standards, which express its concepts, or which reveal its customary ways of manufacturing and using artifacts. Analytic classification focuses on these attributes and, through them, attempts to get at the standards, concepts, and customs themselves. In effect, it attempts to read such modes out of the artifacts.

Not all the attributes of the artifacts are indicative of modes. Some attributes will instead express personal idiosyncracies of the artisans. A unique design, which occurs only once, may be cited as an example. Other attributes fall within the realm of biology, chemistry, or physics rather than culture. The atomic structure of artifacts is an obvious example. The white color of shell artifacts is another. This whiteness does not appear until after the artifacts have been in the ground for some time, it

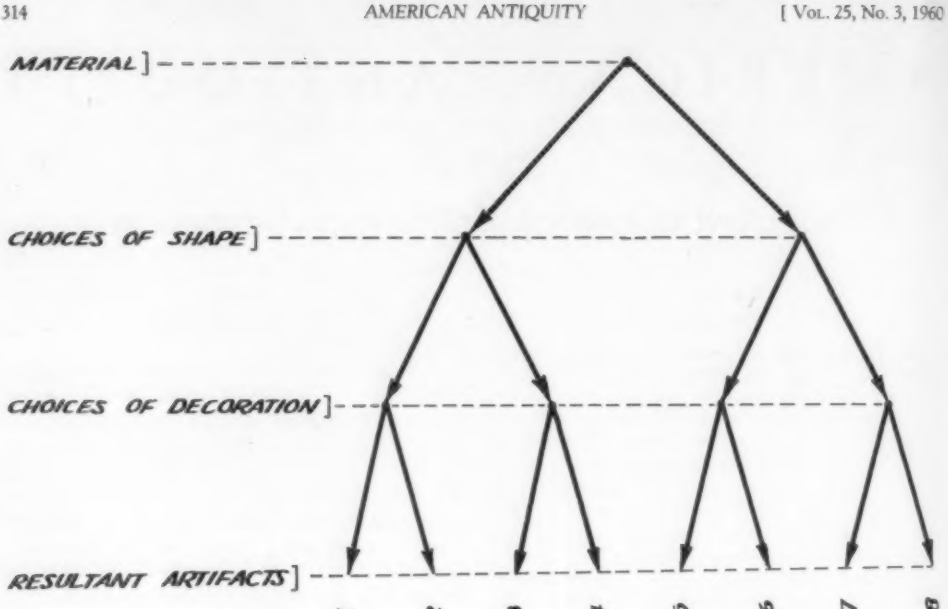

Fig. 1. Procedure of making artifacts.

is as prevalent among natural as among worked shells, and hence it must be considered a purely biological trait which has no part to play in cultural studies.

Analytic classification, then, must single out modes, which are cultural, and exclude those traits which are purely biological, chemical, or physical. One way to do this is to examine a collection in terms of the artisan's procedure, starting first with the materials he used, continuing with his techniques of manufacture, and then considering shape, decoration, and uses. At each stage in the procedure one may find that the artisan had some choice of standards or customs (Fig. 1). This makes it possible, for example, to divide a given collection into one or more series of classes on the basis of the materials used. One can then redistribute the same specimens into other series of classes on the basis of techniques, elements of shape and decoration, and uses (Fig. 2). Each class will have one or more diagnostic attributes, and those attributes will be indicative of a single mode.

For example, an archaeologist may take a collection of potsherds and divide it into two classes, one consisting of sherds with inclusions of grit and the other, of sherds with inclusions of shell. He thereby determines that the potters had two alternative customs of tempering their vessels, one with pieces of stone and the other with pieces of shell. Then, he may pick out the sherds which are from rims and regroup them into a second series of classes, each characterized by a different set of rim attributes. In this case, he will have established a series of standards to which the potters conformed in making rims. He may repeat this process of reclassification with other aspects of material, shape, decoration, and use, ending up, as I have done in the case of my Antillean collections, with as many as 80 modes of material, shape, and decoration (Rouse 1939, 1941, 1952).

It is not necessary, of course, to be so systematic and all-inclusive as this in doing analytic classification. Various authors have concentrated upon technology (Matson 1942), upon shapes (Black and Weer 1936), upon designs (Amsden 1936), or upon uses (C. S. Ford 1937). The important point is that the author be interested in establishing independent modes and not in studying the manner in which those modes are combined on the artifacts.

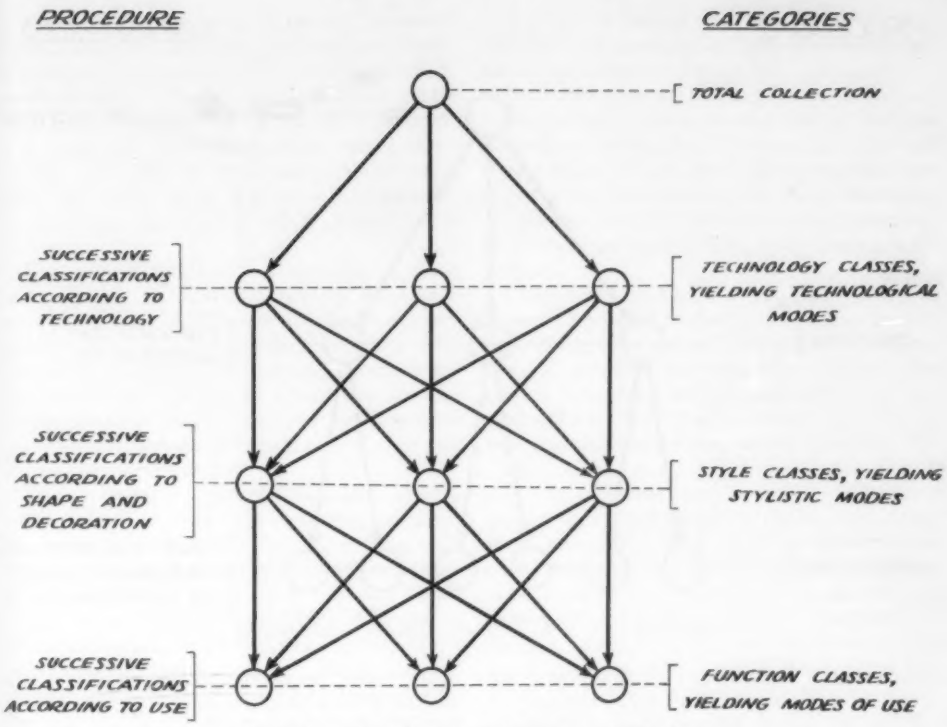
PROCEDURE
CATEGORIES
TOTAL COLLECTION
SUCCESSIVE CLASSIFICATIONS ACCORDING TO TECHNOLOGY
TECHNOLOGY CLASSES, YIELDING TECHNOLOGICAL MODES
SUCCESSIVE CLASSIFICATIONS ACCORDING TO SHAPE AND DECORATION
STYLE CLASSES, YIELDING STYLISTIC MODES
SUCCESSIVE CLASSIFICATIONS ACCORDING TO USE
FUNCTION CLASSES, YIELDING MODES OF USE

FIG. 2. Example of the analytic approach to classification.

The modes may be of two kinds: (1) concepts of material, shape, and decoration to which the artisans conformed and (2) customary procedures followed in making and using the artifacts. In the case of conceptual modes, the archaeologist need only designate one or more attributes of his artifacts to be diagnostic of each class, but in the case of procedural modes he must also infer behavior of the artisans from the diagnostic attributes. The process of inferring procedural modes has been well described and illustrated by other authors (Osgood 1942; Thompson 1958).

Once modes have been set up — whether conceptual or procedural — and their diagnostic attributes have been determined, one may identify these modes on new artifacts simply by looking to see whether the proper diagnostic attributes are present, without actually grouping and regrouping the artifacts. Many archaeologists (Waring and Holder 1945, Fig. 1) have found it helpful to make drawings of the more complex conceptual modes, such as rim profiles or designs, to assist in identification.

TAXONOMIC CLASSIFICATION

We have seen that analytic classification concentrates on the attributes of the artifacts which indicate modes. Taxonomic classification is instead concerned with those attributes which indicate types (Gladwin and Gladwin, 1930, 1931, 1933; Haury 1936; Sayles 1936). As in the case of analytic classification, the attributes indicative of types must be chosen for their cultural significance (Gifford 1960). Indeed, if the archaeologist is being completely logical, he should first do analytic clasification in order to form modes and should then classify taxonomically in terms of those modes, instead of going back again to the original attributes. In such a case, for example, he will use the technique of incision as a criterion for taxonomic classification, rather than the attribute of incised lines. In order to simplify the following discussion, I will assume that the archaeological taxonomist does work in terms of modes rather than raw attributes, in which case a type may be defined as a complex of modes which is diagnostic of a

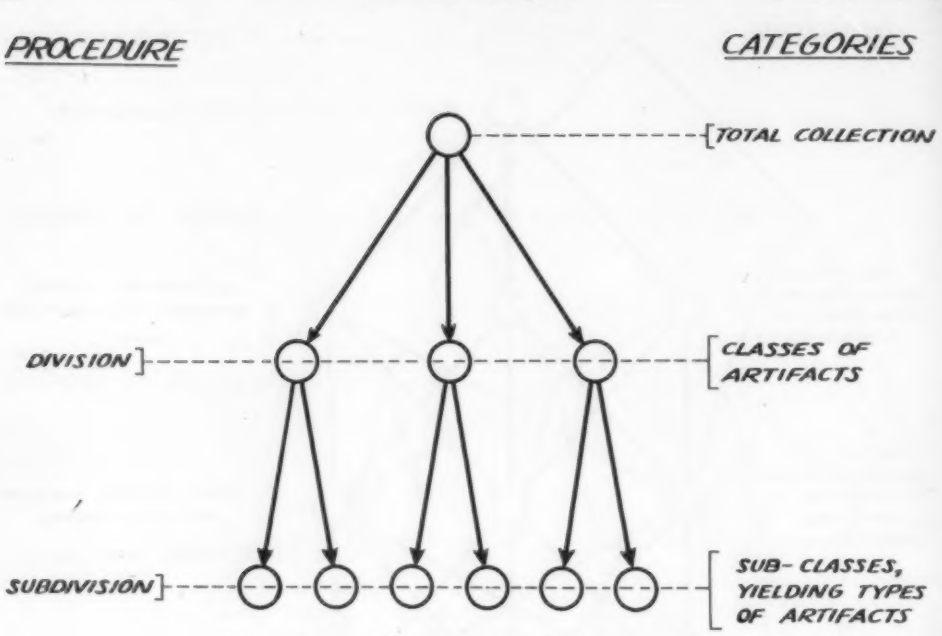

Fig. 3. Example of the taxonomic approach to classification.

certain class of artifacts and which serves to differentiate that class from all other classes.

There are several different ways of classifying a collection to form types. The most systematic one is to divide the specimens into two or more classes on the basis of one set of modes, for example, of materials; then to subdivide each class on the basis of another set of modes, such as shapes; and to continue this process until all the artifacts of the same kind have been separated into a single sub-subclass (Fig. 3). Another way is to work intuitively by simply sorting and resorting the artifacts until they end up in relatively homogeneous classes (Krieger 1944, Fig. 25). A third is to work statistically, for example, by noting the taxonomically significant modes of each artifact on a punch card and sorting out the cards according to the most frequent combinations of modes (Shepard 1956: 322–32). In all cases the end result is the same: a single series of classes or subclasses rather than the successive series which result from analytic classification (compare Figs. 2, 3).

In all cases, the classifier must decide how many modes he is to consider diagnostic, that is, how many are going to end up in the type. He must select more than one, since by definition a type consists of two or more modes. On the other hand, he cannot expect to use all the modes; to do so would result in too large a number of types, especially if the artisan was permitted choices of modes during the course of the manufacture of the artifacts (Fig. 1). The proportion of modes which it is practicable to use as the criteria for taxonomic classification varies with the complexity of the artifacts and with the number of alternatives open to the artisan. Simple artifacts with few alternatives, for example, no decoration, can be classified in terms of almost all their modes, whereas complex artifacts with many alternatives, such as elaborately decorated pottery, require selection of only a few modes from among many. The type "stone ball" may be cited as an example of the former extreme; here the three diagnostic modes, use of stone, grinding, and spherical shape, are practically the only ones which can be analyzed from the specimens. Pottery types illustrate the other extreme; for example, Ritchie and MacNeish (1949: 99, Fig. 36), in their study of pre-Iroquoian pottery, explicitly limited the diagnostic modes they used to rim profiles, designs, and decorative techniques, excluding all the other modes which were analyzable from the sherds.

The personality of the taxonomist may also have an effect upon the number of modes which

he selects to be diagnostic. If he is a "lumper," he will group the artifacts into large and inclusive classes, each of which will contain so much variation that relatively few of its modes can be considered diagnostic. If, on the other hand, he is a "splitter," he will establish many more classes, there will be less variation within each class, and as a result the ratio of diagnostic to non-diagnostic modes will be considerably higher.

The current tendency on the part of some archaeologists to subdivide pottery types into varieties (Gifford 1960) seems designed to meet this problem. It permits one to select a relatively small number of diagnostic modes for one's types and thereby to satisfy the lumper, and at the same time to define varieties in terms of larger numbers of diagnostic modes, thus satisfying the splitter as well.

The distinction between diagnostic modes, which form part of the type or variety, and non-diagnostic modes, which do not, is frequently obscured by the practice of "describing types" (Ford and Willey 1949: 71–8). In the terminology of the present paper, such descriptions amount to a presentation of the modes analyzable from the artifacts of each class. Some of these modes will be diagnostic and, as a result, form part of the type; but others — and in many cases the majority — will not. Ritchie and MacNeish (1949: 100–16) provide a good example. In the case of each type, these authors specify only three "diagnostic features" and then proceed to list a much larger number of other modes of "paste," "surface finish," "decoration," and "forms." The latter modes are not part of the type, as the word is used in the present paper.

The selection of modes has a qualitative as well as a quantitative aspect. The taxonomist must decide not only how many but also what kinds of modes are to be considered diagnostic of his classes and hence constituents of his types. Our colleagues in biology solve this problem by selecting the diagnostics which best show the course of organic evolution. We archaeologists are not so consistent. In the case of pottery and sometimes also projectile points, we select those modes which best indicate differences in time and space (Ford and Willey 1949: 40). Otherwise, we tend to select modes which best express the intrinsic nature of the artifacts (Rouse 1952: 327–9). This difference is reflected in the names of our types, for example, "Glades Plain" in the case of pottery and "semilunar knife" in the case of stonework—or "problematical form" when the nature of the artifact is not known.

This difference must be kept in mind if we are to think clearly about classification. It leads me to make a distinction between (1) historical types, whose modes have been selected, consciously or unconsciously, for their time-space significance and (2) descriptive types, composed of modes referring primarily to the nature of the artifacts.

Once types have been established — whether they are historical or descriptive — they may be used to identify new artifacts without the necessity of actually grouping the artifacts into classes. One need only determine that the new artifacts have the modes comprising a certain type and then apply the name of that type. This is easy enough to do if the types are simple. In the case of more complex types, containing a greater number of diagnostic modes, it has sometimes proved helpful to establish a key to assist in comparison. As in biology, such a key consists of a list in outline form of the modes comprising all of the types. To identify an unknown artifact, one need only trace it down through the key by means of its modes (Colton and Hargrave 1937: 36–41; Colton 1952, 1955).

Another device to assist in the identification of new material is the "type artifact(s)." Not to be confused with an artifact type, this consists of the most typical artifact(s) in a class. To identify unknown artifacts or to check identifications made by means of a key, one may compare the unknown artifacts with the type artifacts and thereby, in effect, assign them to the classes typified by those artifacts (Osgood 1942: 22–5).

MODES *vs.* TYPES

We have now distinguished the two principal ways in which archaeologists classify artifacts, analytic and taxonomic. Analytic classification is done by forming successive series of classes, focusing on different features of the artifacts. Each class is characterized by one or more attributes which indicate a procedure to which the artisan conformed, such as a technique of manufacture, or a concept which he expressed in the artifacts, such as an element of shape or decoration. Each custom or concept constitutes a mode.

Taxonomic classification is done by formulating a single set of classes, which differentiate the artifacts in one's collection according to

type. Each taxonomic class is characterized by two or more modes, selected from among the total number of modes obtainable by means of analytic classification. The modes diagnostic of each class constitute its type.

Types, then, consist of selected modes. We have seen that the nature of the selection will vary from type to type depending upon the complexity of the artifacts, the number of alternatives which the culture offered to the artisans, the personal inclination of the taxonomist (whether he is a "lumper" or a "splitter"), and the purposes for which he plans to use the types.

To put this in another way, modes are inherent in one's collection. If two archaeologists analyze the same collection and do an equally good job of it, they should produce the same modes (Taylor 1948: 129–30). Types, on the contrary, are imposed on the collection. If two taxonomists classify the same collection and decide, for whatever reason, upon different diagnostic modes, they will produce different types (Brew 1946: 46). The mode, therefore, is a natural unit of cultural study, whereas the type is an arbitrary one.

It does not follow from this that types are any less demonstrable than modes. An archaeologist can validate types equally well by grouping them into classes and demonstrating that the artifacts of each class share the same diagnostic modes. The point is that he can then regroup the artifacts according to another set of diagnostic modes and thereby produce other types which will have equal validity.

It should not be implied, either, that a type consists merely of the sum of its constituent modes. The fact that all these modes recur from artifact to artifact gives them a reality above and beyond that of the individual modes.

We have distinguished two kinds of modes: (1) conceptual modes, consisting of ideas and standards which the artisans expressed in the artifacts and (2) procedural modes, consisting of customs followed by the artisans in making and using the artifacts. Conceptual modes are directly indicated by the attributes of the artifacts whereas procedural modes have to be inferred from the attributes. Hence, the distinction between the two is primarily a matter of relative reliability.

Two kinds of types have likewise been distinguished: (1) historical types, formed in order to establish differences of time and space, and (2) descriptive types, formed in order to express differences in the nature of the artifacts. There may be some overlapping between these two, but generally they will be distinct, because the modes comprising each will have been chosen for different reasons. Here, we have a good example of the arbitrary nature of types.

A comparison with ethnology may perhaps help to clarify these distinctions. One of the things an ethnologist does in studying material culture is to ask or observe how his informants make and use their artifacts. Such observations enable the ethnologist to distinguish various techniques of manufacture and uses of the artifacts. In the terminology of the present paper, the latter are procedural modes.

It is likewise possible for the ethnologist to ask his informants to identify the various parts of their artifacts. If the informants go on to distinguish different kinds of handles, for example, or if the ethnologist himself does so, they will be producing the units here called conceptual modes.

A third possibility is for the ethnologist to ask his informants to identify the artifacts as complete objects by saying, for example, that they are "knives" or "scrapers." If either the informant or the ethnologist goes on to define the resultant categories by listing the modes diagnostic of each, he will be producing the units which we have termed descriptive types.

Finally, the informants may refer to certain artifacts as being "old-" or "new-fashioned" and to others as being of a local or foreign style. If either the informant or the ethnologist should define these categories by listing their distinctive modes, he would be producing what are here called historical types.

In other words, the ethnologist as well as the archaeologist may do both analytic and taxonomic classification. The ethnologist, however, is able to do so with the assistance of informants, whereas the archaeologist is forced to rely entirely upon his own judgment and experience in formulating modes and types.

Utility of the Methods

Brew (1946: 65) has observed that "we need more rather than fewer classifications, different classifications, always new classifications, to meet new needs." If Brew is correct, we ought to be able to list the needs for classification in archaeology and to state the kinds of classification which best meet each need. The remainder of the paper will be devoted to this task. It will

be done in terms of the products of our two kinds of classification, conceptual and procedural modes and historical and descriptive types.

Identification of artifacts. There is probably no professional archaeologist who has not, at one time or another, had an amateur come up to him, take an artifact out of his pocket, and ask for an identification. When the amateur does this, he expects to be answered in terms of descriptive types, that is, to be told that the artifact is a "knife" or a "scraper," for example. If the artifact is a potsherd, the professional will probably answer instead in terms of historical types, calling it "Sebonac Stamped," for example. In my experience the latter answer is not so satisfying to the amateur. He does not understand it, unless he is unusually sophisticated, and tends to look upon it as an affectation of the professional. The amateur is probably right in this, insofar as he is interested purely in identification and not in the historical significance of the potsherd. It would be more appropriate for the professional to identify the sherd descriptively as "a fragment of a cooking pot" instead of terming it "Sebonac Stamped."

Interest in such descriptive identification is, of course, not limited to amateurs. The professional must do it in cataloguing his artifacts and in preparing museum exhibits, where specimens are grouped according to types. Descriptive identification is also basic to several more academic pursuits of the archaeologist, which are discussed below. It is fortunate that taxonomy has not lost prestige among archaeologists as it has in the other natural sciences, for it is a vital part of our research.

Determination of the culture of a component. Following the lead of Taylor (1948: 197), I would suggest that, when archaeologists determine the culture of a component, that is, of a culturally homogeneous site unit, they would do well to conform to ethnographic practice. As already indicated, ethnographers handle their collections in two different ways: (1) they identify the artifacts, usually in terms supplied by their informants, and (2) they discuss the manufacture and use of the artifacts (Thompson 1958: 65–146). From the standpoint of the present paper, (1) corresponds to descriptive types and (2) to the modes produced by means of analytic classification. (Ethnographers seem to emphasize procedural modes, but many of them also pay some attention to conceptual modes.) Accordingly, I would suggest that the best way to determine and present the culture of a component is in terms of descriptive types and of procedural and conceptual modes. I doubt that historical types are necessary for this particular purpose, so long as the descriptive approach is applied to pottery and projectile points as well as to other kinds of artifacts, since the historical types will overlap the descriptive types, and anyway they are designed for a different purpose.

The descriptive types and modes will, of course, suffice only to present the material culture of the component. They must be combined with other traits and complexes inferred from the non-artifactual content of the component, for example, the settlement pattern, in order to establish the total culture of the component. The various types, modes, and other traits may simply be listed or they may be grouped into culturally meaningful categories. For example, Fairbanks (1942: 228–9) has organized them about the activities of the people inhabiting the Stallings Island component, in accordance with the concept of activity proposed by Linton (1936: 397).

Classification of components to form cultures. Many archaeologists find it advisable to group their components into cultures, known variously as foci, phases, complexes, industries, or styles. This requires a form of taxonomic classification. The common practice is to compare the components in terms of their "traits" and, as in the taxonomic classification of artifacts, to select certain traits to be "determinants" of each culture (Cole and Deuel 1937: 207–23). When one examines such determinants from the standpoint of the present paper, one finds that they consist of varying combinations of procedural and conceptual modes and of descriptive and historical types. The four appear to be used indiscriminately, without thought as to which units, if any, would be more suitable. To my knowledge, the only author who has attempted to discriminate among the various kinds of determinants is Phillips (1958: 123–4), who advocates the use of types as the primary basis for classifying components but would supplement them with modes.

Phillips does not distinguish among the various kinds of types and modes but, if the suggestions made above for the presentation of the culture of a component are correct, it would be appropriate to use descriptive types and both

kinds of modes. Personally, I would hesitate to state categorically, as Phillips does, that types should be given priority over modes. I suspect that this will depend on the nature of the cultures being studied. In fact, I can conceive of cases in which modes might suffice by themselves. I do not believe that it would do merely to use single modes, but types are not the only available kinds of combinations of modes. It has been possible, for example, to establish structural relationships among modes, such as Amsden's (1936) correlation of "design vocabulary" and elements of shape. Such structural relationships have proved to be effective as determinants in other branches of anthropology, for example, in the classification of folktales (Propp 1958), and I wonder whether they might not work better in the classification of some archaeological components than typological complexes of modes, whether selected for their historical or their descriptive significance.

Alternatively, there may well be cases in which neither types nor modes are the best criteria to use in classifying components. Nonartifactual traits, such as settlement patterns and means of subsistence, may sometimes prove to be superior.

In any case, I am certain that artifact types alone cannot provide an adequate basis for the classification of components, if only because they do not cover enough of the culture of the components. For example, Brew (in Tax and others 1953: 245) has argued that there is no such thing as a Folsom culture (complex) because practically nothing is known of it but the Folsom type of projectile point. Even assuming that we have only the one type of point, the situation looks better when viewed in terms of modes and non-artifactual traits, for we know the kinds of material used; the techniques of manufacture; distinctive elements of shape; a characteristic, if assumed use; and something about the means of subsistence. All these are separate traits which Brew obscured by limiting himself to the type concept.

Dating of components and cultures. Historical types are unquestionably the most effective kind of unit to use in dating components and cultures. The fact that their constituent modes have been selected for time-space significance makes them superior to descriptive types; and the fact that they consist of multiple modes gives them greater reliability than individual modes. This is true whether the types are used as "time markers" with which to correlate strata (components) in the manner of paleontology or are studied in terms of their relative popularity in a more purely archaeological fashion (Heizer 1958: 112–3, 114–5).

Definition of cultural periods. By the same token, historical types should be most suitable for defining local cultural periods. J. A. Ford's various chronological studies may be cited as examples; his lettered "time scale" and the correlated periods are measured against the frequency changes of pottery types (Phillips, Ford, and Griffin 1951, Figs. 17–21). If, on the other hand, one wishes to work with chronology on a regional, multi-areal basis, modes are likely to be more practicable, whether used singly or in combinations, since they tend to have a broader distribution than types (Cruxent and Rouse 1959).

Studies of cultural distribution. Following J. A. Ford's lead (1952: 319), I would suggest that modes are the best unit to use in studying cultural distributions. One may trace their persistence and their relative popularity through time (Rouse 1939, Fig. 6) or their diffusion from area to area (Wendorf 1953: 163–70). By so doing, one will not only be able to reconstruct the histories of the individual modes but also one will find that certain modes tend always to occur together and hence to form discrete historical complexes. Some of these complexes will correspond to types, that is, they will consist of the same modes as those comprising types, but other complexes will cut across the types. The structural relationships discussed above provide one example. The "traditions" and "horizon styles" of Peruvian archaeology are another (Willey 1945).

An archaeologist who studies the distributions of individual modes, then, will be in a position to reconstruct the histories of those modes and also the histories of types and of nontypological complexes of modes. On the other hand, the archaeologist who studies only the distributions of types will be unable to get at the histories of the other kinds of units. Southwestern archaeology, where it is customary to concentrate on the distribution of types (Colton and Hargrave 1937, Fig. 1), is a case in point. Several authors (Rouse 1954: 223) have commented on the lack of horizon styles in the Southwest. This may well be a peculiarity of the culture history of the Southwestern area, but there is also a possibility that it reflects fail-

ure on the part of Southwestern archaeologists, in their preoccupation with types, to trace the distribution of enough individual modes.

Studies of cultural change. If one is interested in the problem of how one complex (whether typological or not) or one culture has changed into another, modes are again the proper unit to use. As Barnett (1953: 185) has put it,

> in order to understand the innovative process, we must be prepared to analyze ideas in any fashion and without limit . . . so that we may follow the ramifications of recombination as they actually occur. We cannot deal with the gross stereotyped wholes [i.e., types] only. . . . We must treat conventional ideas, such as those of tables and men, merely as more or less stable organizations of experience that can be torn down and reassembled in the wink of an eye.
>
> In all the sciences we have come to expect very detailed analyses of data for purposes of classification. This lead, however, has not been followed by students of [culture change]. . . . In trying to understand invention it is common practice to deal with such gross units as automobiles and buggies or [pottery types] and spinning wheels. The attempt to understand one of these complex wholes in terms of the whole of another will give us no insight into their true relationships. We must view the inception of each one in terms of an analysis of its component parts.
>
> The linguists long ago recognized this necessity, and students of linguistic change do not hesitate to break down sentences, words, parts of words, and parts of these parts. . . . There can be no question that linguists are far ahead of the rest of us in their understanding of the mechanics of cultural change.

Barnett might have added that archaeologists are also accustomed to do analysis and to study culture change in terms of the resultant modes. Gladwin's (1957: 282–4) final report on Southwestern archaeology may be cited as an example. Linguists cannot claim sole credit for this kind of approach, nor to have produced the only significant results from it.

CONCLUSIONS

In the foregoing section I have attempted to indicate some of the situations in which it is better to use modes, resulting from analytic classification, or types, resulting from taxonomic classification. I do not pretend to have fully covered the subject; rather I have intended to call attention to it as a problem which deserves the consideration of archaeologists. Too many of us, in my experience, fail to discriminate between modes and types, treating them as equivalent units and substituting one for the other whenever it is convenient to do so (Webb and Snow 1945: 16–28). We need to think more about when the various kinds of modes, types, or combinations thereof will be the most effective units to use.

This paper has likewise been intended to call attention to the way in which analysis and taxonomy complement each other as methods of classification. I think we should beware the tendency of some archaeologists to become preoccupied with one to the neglect of the other.

As already indicated, taxonomic classification has received particular emphasis in the American Southwest. We appear at the moment to be witnessing the diffusion of certain Southwestern taxonomic developments to the Southeast (Phillips 1958) and Mesoamerica (Smith, Willey, and Gifford 1960). I do not criticize this diffusion; indeed, I would recommend that it be expanded to include other Southwestern taxonomic devices, such as the key and the type artifact.

On the other hand, I share with Sears (1960) the belief that the analytic approach likewise needs to be strengthened and applied more widely. We need more studies of individual modes and of their non-typological combinations, such as the recent work of Wasley (1959) in the Southwest.

The fact is that both analytic and taxonomic classification must be done in order to make a full study of any collection. Neither, by itself, will supply a complete picture of the culture of the collection, nor will either be able to furnish all the data needed to formulate cultures or to reconstruct culture history.

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CERAMIC SYSTEMS AND EASTERN ARCHAEOLOGY

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ABSTRACT

The system of ceramic taxonomy presented by Phillips for use in the Southeast is considered to be too formal and rigid. While it attempts to serve the same functions as the currently used concepts of type and variety, the proposed new system would be considerably more complex in its application. The ceramic type, as currently defined and used, is well adapted for establishing skeletal chronicle. Any modification would lessen its usefulness. A greater emphasis on including descriptive classes in reports is offered as more valuable than the adoption of a formal taxonomy. The content of the classes cannot be determined prior to description, whereas the formal taxonomy prescribes the content of each taxonomic unit. These classes would increase the utility of sherd collections by making available data which can be applied to problems, such as those of process, for which neither the type nor the proposed new varieties are suitable.

PHILIP PHILLIPS has recently (1958) suggested a modification of the system of ceramic taxonomy proposed by Wheat, Gifford, and Wasley (1958) for the Southwest, a modification which he believes would be of service in the classification of pottery in southeastern United States. I cannot agree that the proposed system would be of value and have become convinced that formal taxonomy of any greater degree of complexity and rigidity than that we now use would produce more work and neither an increase in knowledge nor greater facility in problem solution. This may be in part due to an antipathy toward taxonomy as opposed to analysis. The taxonomic units are, or should be, products of analysis. Above the minimum level of the mode, the dimensions of the units are conditioned by problems of prehistory and of communication.

As I understand Phillips he is proposing that the term *type* be reserved for a definable entity which has some breadth or inclusiveness in decorative motifs, technique of execution, or total design. Such a type will be composed of varieties which are those subdivisions of the type, or those components, which achieve terminological recognition. In the examples given, and in proposed practice, the varieties are "technological varieties in decoration" (Phillips 1958: 120). If these are simply recognized, but not demonstrated to have "temporal or local significance," they are cut back to modes. Varieties are then modes of technological variation in decoration which are named if they are time and space markers. I infer that technological variants in decoration which do not achieve the status of varieties are described without being named. Types are organized into systems on the basis of a shared element of general style, such as zoned rocker-stamping or parallel-line incision horizontal to the rim. Phillips (1958: 120) gives the background for the evolution of this concept in the Hopewell Zoned Stamped pottery.

Certainly there can be no serious objection to the concept of a ceramic system since, unless I am greatly mistaken, most eastern archaeologists have been so grouping pottery for years, and designating their products descriptively. References to "fabric-marked pottery," "check-stamped pottery," "fiber-tempered pottery," and so on are too numerous for explicit references to be necessary. That is, there is no objection unless we attempt to freeze the limits of the concept formally. A system of shell-tempered as opposed to a system of grit-tempered pottery is often a useful concept, but certainly these systems cannot be fitted into the same formal taxonomic limits as a Hopewell Zoned Stamped system which is based on variation of the technique used to produce an effect within a single highly similar style.

I agree with Phillips (1958: 123) that:

> . . . the primary aim of ceramic taxonomy is to facilitate the daily operations of archaeology, such as stratigraphy and seriation, by means of which archaeological units (components, phases, cultures, and so forth) are isolated and compared.

Any disagreement here would depend on whether the term "primary" is used in the sense of sequence, or of importance.

I cannot agree with the rationale which Phillips offers for the formal taxonomy. This is in part due to the re-definition of ceramic complex, a key concept in his discussion, as ". . . the sum total of modes and varieties (less often types) that comprises the full ceramic content of an archaeological unit, usually a component or phase" (Phillips 1958: 123). This definition contrasts strongly with general usage as made explicit in definitions by Willey (1949: 6), by Phillips, Ford, and Griffin (1951: 64) and

others, which are in terms of types, grouped on the basis of recurrent association in a specific area within a specific period of time. Relationship is not on the basis of common possession of individual modes, as Phillips suggests (1958: 123). This has usually been the principle used for grouping types into series. Rather, the basis for a complex is recurrent association, and as so defined and used by most eastern archaeologists seems to me a perfectly useful tool for the elucidation of certain classes of space-time problems. It might be well to point out that very few Southeastern "cultures," components, phases, or periods are anything but ceramic complexes. What else, for example, are Tchula, Troyville, or Deptford?

It seems to me that Phillips' proposal boils down to a system of pigeon holes for the classification of data in a hierarchical ranking, with dimensions predetermined by factors of technology in design execution and by space-time problems. I think that any taxonomic system which starts by assuming the existence of units of directly comparable dimensions is also making an assumption of comparability of archaeological, or cultural data with biological data, and is then assuming that there is an analogy between biological and cultural systems. That there is any real comparability I do not believe, for the reasons discussed by Brew (1946) and by Rouse (1960).

Analysis is "the resolution of a compound into its parts or elements; the act of ascertaining, separating, or unfolding in order the elements of a complex body, substance, or treatise" (Funk, Calvin, and Vizetelly 1952). Analysis should produce classes of varying degrees of inclusiveness, classes which will overlap extensively. That the degree of inclusiveness of these classes should be determined in advance I do not believe, except through their applicability to specific problems. J. O. Brew has made several statements which seem particularly relevant here.

> We must not be satisfied with a single classification of a group of artifacts or of a cultural development, for that way lies danger and defeat. We are, or should be, in search of all the evidence our material holds. Even in simple things no single analysis will bring out all of that evidence. (Brew 1946: 65.)
>
> And the dangers will be pointed out, in the so-called ideal or comprehensive schemes of overemphasizing single criteria or selected groups of criteria which although of value in studying those particular characters, can not fail to give a warped picture of the whole. (Brew 1946: 47.)

The proposed formal taxonomy, as an ideal or comprehensive scheme based on selected criteria in ceramic decoration, is subject to Brew's warnings. Brew does not decry classification, nor do I. The objections are to systems imposed on data, rather than being developed from problem-oriented multiple analysis.

I might state here my views concerning some of the purposes of ceramic analysis and the function in archaeology of its products. Actual analysis of archaeological collections can be done in many ways, but no matter which of us does it or which technique is used, it usually involves sorting for all of the modes which our experience has taught us to recognize. We inevitably produce far more units composed of single modes and clusters or groups of modes, than we can handle in "types," whether the analyzer is a classificatory "lumper" or "splitter." The varied classes resulting from the analysis may be refined, abstracted, and otherwise manipulated into types, left raw as descriptive classes, or partially re-worked into in-between grades. These classes of data serve, or should serve, two major functions:

1. To provide units, susceptible to simple statistical handling, which the analyst may apply to the solution of his problems. Pottery types in the southeast have become flexible tools for the problem of ceramic complex definition and space-time alignment.

2. To provide a body of data, describing what was actually found, which may be used by other archaeologists in attempts at the solution of the problems with which they may be concerned.

Data are organized under the first of these functions for application to immediately perceived problems, while the problem under the second is preservation and communication.

The final products of analysis remain much as they are now under the Phillips modification of the Wheat–Gifford–Wasley system. That is, as technological variations in decoration are recognized to have temporal or local significance, they are given names, and Phillips (1958: 120–2) gives excellent examples in rocker stamped and incised pottery. This is what is done now with types. Current procedures of problem definition, problem solution, and communication are often affected only by changes in names. However, we have types which combine various sorts of modes because these combinations have proven to be effective

for work on current problems. To my mind, the proposed system imposes a superstructure of taxonomic rigidity and greater complexity on current procedures of phase and ceramic complex recognition, definition, and time-space alignment of the defined units. I cannot then agree that we need a "strictly formal taxonomy right up to the top" (Phillips 1958: 124). This assumes that formal, comparable units exist which will fit into a rigid structure of relationship, the biological analogy. Such a formal structure, if practicable, would not facilitate solution of the problems now adequately handled by flexible concepts of the ceramic type and the ceramic complex.

I think that part of our current difficulties in ceramic analysis, difficulties which have lead to such revisions of the type concept as that offered by Phillips, are due to a tendency to use types for problems to which they are not suited. Rouse (1960) points up sharply some of the objections to this in demonstrating that there are a great many tasks the type cannot do, but that the mode, the class, or specially devised classificatory units can.

Another fact of some importance is that the type concept has, in the hands of some of our best eastern archaeologists, undergone a quiet, almost unnoticed evolution in recent years. It has become a refined tool for the phase definition and problems of temporal order which have been the major problems of eastern archaeology. Krieger (1944: 124) points this out:

> Any group which may be labeled a "type" must embrace material which can be shown to consist of individual variations in the execution of a definite constructional idea; likewise, the dividing lines between a series of types must be based upon demonstrable historical fact, not, as is often the case, upon the inclinations of the analyst or the niceties of descriptive orderliness.

The emphasis on historical problems as a determining factor in classification and the strictures against typology for purposes of description are in accord with general practice. Ford (1952: 321) carries this a step further:

> The typologies must be directed toward the solution of the same problems. Measurements of cultural influence become difficult; if not impossible when one of the measuring devices, the type, is defined to include two or more of the streams of prehistoric cultural concepts that have been isolated in an adjacent region.

This concept of the type as a tool for limited and specific kinds of problems, and its inherent, if unstated, limitations for other purposes are brought out rather dramatically by the following statement:

> When Gordon Willey began his survey and stratigraphic work on the northwest coast of Florida in 1939, he had had previous experience both in the Louisiana and Georgia areas and was thoroughly familiar with the ceramics of both. Thus he was able to *design* the northwest coast of Florida classification system so that it not only *controlled* time change there but also measured the common cultural connections between that region, Georgia, and Louisiana. (Ford 1952: 321; italics mine.)

My point that pottery types are not primarily descriptive devices but are refined tools for the elucidation of space-time problems could be further documented by references to a long list of archaeologists who have played their part in this conceptual evolution, and who have played the dominant roles in the space-time systematization of eastern archaeology in the last 20 years. Without this concept, little of the progress in the definition of ceramic complexes and the temporal alignment of these units would have been possible. We might even have stopped at the check-off trait list level and have gone out of business by now.

Since the pottery type has grown, through careful nurture, into a good and useful tool for certain jobs, any projected change must demonstrate that it will do a better job of space-time systematics. Otherwise, and I feel that Phillips' suggestion suffers from this fault, the utility of the present flexible tool will be limited. Refinement in manipulation of this tool continues, such as Ford's handling of surface collections and the suggestions for the application of statistical techniques by Spaulding and others.

A very good example of what is done with types can be found on the Northwest Gulf Coast of Florida. Here Willey erected a chronology for what was then a new area. He recognized rocker-stamped pottery and demonstrated an early stratigraphic position for it (Willey 1949). A type, Alligator Bayou Stamped was defined, based upon these sherds. By definition, the type included clay and sand temper and plain or dentate rocker stamping outlined by broad-line incision (Willey 1949: 372–4). With only a few sherds to work with, in terms of the problems of the area and period, it could only be the total collection which was important. Yet this, and similar collections made and classified since, include a fair number of distinguishable types, as they are now defined, plus other variants as yet not distinguished by type status. Included are (most of the sherds

were checked against good type collections as well as the type descriptions):

Troyville Stamped (Restricted definition — Ford 1951: 49–50).

Jonesville Stamped (Greengo 1957; Phillips, personal communication).

Marksville Stamped — possibly (Ford and Willey, 1940: 65–74).

Sand tempered sherds otherwise identical to the above.

Clay and sand tempered sherds with zoned dentate, but not rocker stamping.

Mixed temper varieties of all three decorative variants.

My point here is that, operationally, there was nothing at all wrong with Willey's classification. He separated out a set of modes from his sherd classes which, fined down into a type, and as a part of a ceramic complex, served very well as a tool in defining a restricted period in the Northwest Florida Coast area. But, this type cut into two "varieties" as Phillips (1958: 121) suggests will not serve any better for period alignment, and will not provide usefully classified data for work on such current problems as the relationship between cultural units on the Gulf Coast, or the direction of movement of certain ideas. The current type served my first function above in serving as a tool for the solution of problems of complex definition and sequence; neither it nor the proposed revision will serve the second function, that of providing data which can be used for other problems. There are many other types which would have served to illustrate this point. This one was chosen because I have worked with the collections and because it was used as an example by Phillips.

The extant collections can, of course, be re-analyzed, and I have done this, in part or whole with those collections containing the Alligator Bayou Stamped sherds. This analysis has made it rather obvious that the first rocker-stamped, and much of the first incised, pottery in the Florida Northwest Coast is clay-tempered, so that the "Hopewellian" tradition and the accompanying ceramics representative of the earlier stages of the Gulf tradition are coming from the lower Mississippi Valley into this area, there merging with the resident sand-tempered and stamped traditions. Certainly some amount of re-analysis will always be necessary. It does seem though, with the specter of huge piles of sherds to be re-analyzed confronting me, that further transmission of the classes of data, isolated during analysis and then boiled down into types, is possible and necessary.

Because the tool most suited to the problems of skeletal chronicle is mode of decoration, types have been based predominantly on such modes. It is very difficult, with such exceptions of varying degree as those in Ford's Greenhouse report (1951), Krieger's tables in the Davis site report (Newell and Krieger 1949) and the Hiwassee Island report (Lewis and Kneberg 1946) to find any quantification for modes of vessel shape, base and rim form, appendages, and so on. And, because the problems of yesterday are not those of today, it is just as difficult to find quantitative data on the technological and decorative modes included in a single type.

The statement has been made that ". . . history — and prehistory — is both the space-time scale and the content and processes which it measures" (Phillips and Willey 1953: 616). I am suggesting that pottery types, properly defined and used, are tools for measuring the space-time scale. They are not adequate for the study of content and processes, and cannot serve both functions. It may be further suggested that in our self-destroying and ever under-financed field of study, that description of what we find must supplement the typology which we use for work on skeletal chronicle.

In ceramic analysis, many classes are isolated. We can do what Krieger (Newell and Krieger 1949) and others have done to a degree, that is, incorporate description of the classes resulting from the first analysis into our reports along with the formal typology devised for application to our immediate problems. Varieties will not do this descriptive job because they are modes or minimum modal clusters whose content has been determined by current theories and problems of temporal and areal systematics. Modes will, by definition, serve this descriptive function.

Since analysis is an investigatory procedure, the present flexible type concept has definite merits and should continue. The stamped ware of the Etowah valley offers a good example. The major problem revolves around:

1. Mode of vessel form. Limited to simple open bowls and jars with gently flaring rims and rounded bases.
2. Two temper modes, sand or crushed shell.
3. A single mode of decorative technique, stamping.
4. A dozen stamped designs. By far the commonest are line blocks, ladder diamonds, two bar diamonds, filfot crosses, with others represented only by fractional percentages in any collection (Fig. 1).

All sorts of types or varieties are possible here. Yet a group of us, working on common

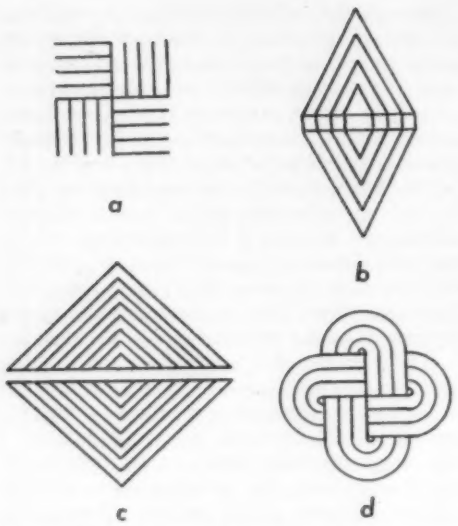

Fig. 1. Common designs in Etowah and Hiwassee complicated stamped pottery. *a*, line block; *b*, ladder diamond; *c*, two-bar diamond; *d*, filfot cross.

problems, discovered that our immediate ends, the definition of complexes and their ordering in time, were best served by using two types. One is "Hiwassee Complicated Stamped" with shell temper (Lewis and Kneberg 1946: 104); the other, with sand temper, "Etowah Complicated Stamped" (Wauchope 1948). The type differentiation here, based solely on temper, does not seem to have any consistent temporal status. Quite possibly it will reflect, in intensity of the Hiwassee type from site to site, the importance of and intensity of contacts with the Hiwassee and Dallas cultures to the north. Temporal differences are in terms of relative frequency of design units, stratigraphically verified at a number of sites. These designs are certainly at least as distinctive as plain and dentate rocker stamping. I suggest however that these simple descriptive terms, supplemented by adequate illustration, do our job in a more workmanlike fashion than a fixed and formal mode of nomenclature which would force us to break this material up into eight named types, or two types with four named varieties each, or one type with eight named varieties. Actually, since there are many of the readily sortable but numerically less important motifs, which we also handle descriptively, the problem would become even more involved.

I should emphasize that this is not meant to be a recommendation for another formal method of procedure, but is simply an example to indicate that flexibility of procedure is desirable. The problems of analysis in each case are different, and the product in terms of types and other units which should be described will differ with the problems.

I am here, I think, restating a case which Rouse has stated clearly and explicitly:

> It will be convenient to distinguish between two products of classification: The *class*, or series of objects which are grouped together, and the *type*, or series of attributes which distinguish them as a class. If one's principal objective in classifying is to produce a description of artifacts, . . . , one is likely to concentrate on the class and to present its characteristics in some detail. If, on the other hand, one is primarily interested in defining units of culture for use in distributive studies or for some other interpretative purpose, then one will probably emphasize the type at the expense of the class. (Rouse 1953: 62.)

My suggestion then boils down to more description of the classes to supplement the types which are referrents for specific problems.

The suggestion was made earlier that this description is best done in terms of "modes," using Rouse's definition of this concept (Rouse 1939: 11), and that interareal contacts can best be traced through the spread from one region to another of single modes, such as the clay temper on the Gulf Coast and the shell temper in north Georgia. Pottery types do not appear to spread as complete entities, significantly, unless one population overwhelms and, usually, replaces another.

Realization of new kinds of problems have given us new tools of general utility, tools which have been devised for the problems and from them. The horizon style and the tradition (Willey 1945: 33) are two such. Each, as I see them, can really be visualized as tracing certain modes or specific patterns of modes through space in a limited temporal dimension, or through time in a limited spatial dimension. Ford's use of "tradition" (1952) is similar in that it, too, does not deal with types as such, but with patterns or clusters of modes of varying degrees of inclusiveness, sorted from available data to shed light on the problems followed. This particular job could not have been done with any other units, no matter what they might have been called.

Obviously, if flexibility is desirable, and if description is of classes of modes which the archaeologist's grasp of his business tells him are

of potential utility, an explicit scheme for description is impossible. It may be that Rouse's modal analysis has failed of wider acceptance simply because of this necessary lack of explicit directives, its failure to achieve the status of a "do-it-yourself" manual for archaeology. There are, of course, techniques for presentation of masses of data in condensed form. Tables, charts, and graphs have all been used successfully and can continue to serve these functions. Narrative description may be the only way and is often the best way. Other techniques will suggest themselves. The only certain point is that the dimensions and content of the classes should be determined after analysis, not before. Extant types will very often do the entire job of problem solution and data presentation, but not always.

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THE TYPE–VARIETY CONCEPT AS A BASIS FOR THE ANALYSIS OF MAYA POTTERY

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ABSTRACT

The method being used to analyze pottery from Uaxactún and Barton Ramie by the application of the type-variety concept is offered as an analytical approach well suited to the classification of Maya ceramics. Types and varieties are seen as the best archaeological approximation of the ceramic abstractions which existed in the prehistoric cultural configuration. The systematic application of the type-variety concept will make it possible to establish analytical ceramic units which will be comparable throughout the Maya territory, to undertake detailed chronological and areal studies, especially in areas away from the ceremonial centers, and to use ceramics as a step toward cultural interpretation. Considerable attention is given to the procedure of analysis and to the problem of naming the resulting analytical ceramic units. The most desirable nomenclature is illustrated by Aguacate Orange [type]: Holha Variety. Place names have been used for the primary type term and for the variety name, but a descriptive term is used for the second part of the type name. The desirability of keeping the variety flexible and free of bias or prejudice stemming from the nomenclature is stressed. The variety is the smallest meaningful unit of classification in the type-variety method. Sorting, naming, and tabulating begin with varieties which, in turn, lead to the recognition, naming and description of types.

DETAILED STUDIES of Maya ceramics began with Vaillant's important pioneer work in 1927. Since that time other students have followed, in major outline, the kinds of pottery description, categorization, and presentation which he laid down. These procedures have established broad classificatory units called "wares," many of which, from an analytical point of view, are comparable to the ware concept in archaeology of southwestern North America (Colton 1953: 51–8). According to this usage the pottery within a ware displays a consistency in such technological attributes as paste or surface finish. Surface finish has been given particular emphasis by those who have dealt with pottery from the Maya subareas. Once wares keyed largely to surface finish or allied criteria were established, vessel shape was used for a first level of subdivision. Various features of decoration as well as other attributes have been assigned most frequently a somewhat "lesser" position in the typological hierarchy. Different scholars have not followed, however, a standard set of procedures, and utilization of "ware" and "type" has differed greatly with regard to the scope of their boundaries and in specific applications (Ricketson 1937; Thompson 1939; Shepard 1948, 1956; Wauchope 1948; Longyear 1952; Smith 1955; Brainerd 1958). A concept of the pottery type, more comparable to that presented in this paper, has been used elsewhere in Mesoamerica, although not in the Maya territory (Ekholm 1942; Tolstoy 1958; MacNeish 1958).

There has been a very noticeable trend toward more intensive and rigorous analysis and toward systematization. In keeping with this trend, and in an attempt to render pottery as effective and refined a tool as possible for the plotting of culture history and the dating of phases, we offer at this time some suggestions for Maya ceramic analysis. These suggested revisions have occurred to us in the course of a review and analysis of the large pottery collections obtained from the Maya sites of Uaxactún in Guatemala and Barton Ramie in British Honduras, and as a result of discussing our problems with colleagues. These suggestions are grounded, to an extent, in the knowledge that the type-variety concept has been successfully employed in both eastern and southwestern North American archaeology. In addition, our interest in applying the type-variety concept to Maya pottery has been heightened by a desire for greater knowledge of Maya settlement in locales away from large ceremonial centers. In such studies a means whereby approximate dates can be affixed to the different occupation periods in ordinary house mounds is essential. Pottery, when classified into types and varieties that are diagnostic of particular ceramic complexes that in turn are attributable to specific phases, provides the best and most accurate way of achieving a relative or correlated date for time periods represented in the debris of a house mound.

In the initial studies of Maya pottery conducted as part of the research program of the Carnegie Institution of Washington, it seemed wise to proceed along relatively broad lines of approach. As a result formulations such as ceramic phase (Mamom, Chicanel, and so on), ware, shape, decorative technique, and style of

design were used in an inclusive fashion to portray ceramic findings. Although less precise as to definition than the concepts proposed here, each ceramic phase had wares that were peculiar to it, and each ware had particular forms, techniques of decoration, and styles of design. Furthermore, whenever the evidences seemed to warrant so doing, wares were broken up into types such as monochrome Fine Orange (Y, Z, X, V, U; Smith 1958) and Plumbate (San Juan, Robles, Tohil; Shepard 1948). This method has not been tremendously successful largely because no two people followed the same definition or concept of ware and type and except for certain technological studies conducted by Shepard, paste and temper analyses have largely been neglected.

Previous to the work at Uaxactún, pottery had not in itself been used for dating purposes in the Maya subareas. As a rule, in the great sites that received attention, architectural features and monuments provided dates that could be directly associated with fill or tomb deposits yielding pottery. Since the deposits had already been given a time value, the job that remained, as it pertained to pottery, was in most instances descriptive (Willey 1958). But in order to employ pottery effectively as a dating device, in cross-cultural comparison, and as an element of synthesis and interpretation, it must be described consistently by using defined units of analysis. We feel the type-variety concept offers the analyst a way that is not only systematic but that recognizes ceramic units which approximate those in vogue among the prehistoric populations under study.

There has been an earnest endeavor on our part to be consistent and exacting with regard to the archaeological nomenclature used in setting forth the revisions suggested here. In this connection Mesoamerica is considered a culture area. The territory of the Maya is thought of as essentially two subareas (Maya lowlands and Maya highlands) within the Mesoamerican culture area. The Maya territory as a whole is often referred to as the Maya subareas and each subarea is composed of regions such as the Petén (Willey and Phillips 1958: 20–1).

We have been especially careful in our use of the term "complex." Complexes are gross and inclusive analytical units in that each descriptively encompasses all the material of a certain kind that is known from a given phase. Reports concerned with material culture are normally organized and written along these lines so that a lithic complex, a bone complex, and so forth, is presented for each period of time and each archaeological culture represented at a site. Consequently a ceramic complex, a lithic complex, an architectural complex combine with other complexes to constitute the artifactual manifestations of an entire phase and when considered together as a whole they represent the total material content of a phase. Consonant with this distinction, we speak of a particular ceramic complex but never a ceramic phase, because a phase is made up of all that is known of a cultural configuration during a particular interval of its prehistory of which the pottery is only a part.

It will also be noticed that individual features, such as paste, rim shape, surface color, and all other observable criteria found to have come together in a variety are called "attributes" rather than "modes." As a result the term "mode" has been momentarily emancipated with a view to sharpening its definition. A mode, then, becomes the term designating a ceramic attribute (or collectively a small group of inseparable attributes) that has been observed to have singular import and meaning beyond that of any purely descriptive aspect because it appears in several or a number of different varieties (of different types) remaining all the while unaltered in its own essential characteristics. A mode by our definition is an attribute or cluster of attributes that displays significance in its own right. Modes should be the subject of separate study to view their individual behavior in cross-cutting varieties and types through time and space. Any collection of pottery is best described in terms of varieties and types as well as modes in order to present a comprehensive picture. The type-variety study should be integrated with the mode study and neither should be conducted or set forth in a mutually exclusive manner. Rowe's use of the term "feature" is identical to our use of the term "attribute." He indicates that

> a feature is any characteristic or detail of an object which can be observed and isolated, whether of material or workmanship or decoration. Features acquire significance with relation to a particular problem, however, so that not all the features that can be observed are equally useful for a given purpose (Rowe 1959: 4–5.)

Features, or attributes as we prefer to call them, that can be shown to possess a particular significance of their own we distinguish by referring

to them as modes (the "Haynes Bluff" rim mode for example).

Recently Phillips (1958) has outlined his idea of the type-variety concept as it might be applied to eastern North American pottery analysis. Wheat, Gifford and Wasley (1958) have also presented formalized definitions and offered a brief explanation concerning the usefulness of ceramic varieties in the analysis of southwestern North American pottery. The Phillips concept of pottery type and the Wheat-Gifford-Wasley concept of ceramic variety, when combined, appear to be an effective method for the laboratory analysis and description of prehistoric pottery. The merger of these two schemes we refer to as the ceramic type-variety concept. The type-variety concept embodies a methodological approach that can be employed to considerable descriptive advantage while at the same time it is a means whereby ceramic data can be ordered in such a way as to yield concrete information regarding cultural processes. Pottery that has been properly treated within this frame of reference can indicate with reasonable accuracy relative phase boundaries, ceramic distributions, cultural interrelationships, the nature of indigenous ceramic traditions, to mention but a few aspects. Without the analysis of pottery in a standardized fashion, by the recognition of attributes and the combination of these attributes into varieties and types, efforts in the direction of synthesis are difficult, if not impossible. Types and varieties are in themselves a first step in synthesis, being directly linked with the basic observable data as seen in attributes.

Inasmuch as a type (or a variety) represents a clustering of attributes, it is an abstraction, but it is an abstraction that mirrors a particular cultural trait. The word abstraction is used in the sense that no one vessel or single series of vessels represents the type in its entirety. A type can be abstract and yet in its description approximate the range of variation encompassed by the original prehistoric ceramic unit. Pottery types are the material documentations of cultural phenomena and are, therefore, entities which can validly be compared cross-culturally. It is our position that varieties and types were realities within the cultural configuration of their origin and it is our job as analysts to recognize these ceramic entities. In recognizing the types that are present in a phase, we get at the ceramic manifestations of a culture in a specific region at a certain period of time as best we can archaeologically. The content of any one ceramic complex is, therefore, what is known of the pottery in a culture of a region during a particular interval of time. As such it is directly comparable to what is known of the pottery in another culture or another element of the same culture in a different region during a given time span *if* that pottery is documented on the same level of abstraction and in the same terms of synthesis (types and varieties in a complex).

Attributes are observable criteria which, when combined, are seen first as varieties, then as types. An attribute by itself is no more than one distinguishable feature. Temper (a single attribute) is only temper by itself; but temper, a method of firing, surface finish, form, and other attributes when viewed together become a definable entity and represent a real element in the material inventory of a culture. And when elements of like character are placed side by side, a type becomes evident because the analyst has been able to discern the combinations of attributes which fulfilled the ceramic desires and necessities of a society in a certain region at a certain time in prehistory.

These points have been briefly touched upon in order to emphasize the necessity for dealing with Mesoamerican pottery on a type-variety basis as well as by giving due consideration to those conspicuous and important modes that may represent horizon or phase "markers." The pottery recovered from the ceremonial center of Uaxactún is one of the most extensive collections ever made. Although it was examined from viewpoints other than the type-variety concept, it has been subjected to detailed and intense treatment to an extent not heretofore realized in the Maya subareas and the results have appeared in published form (Smith 1955). The collections from Barton Ramie in British Honduras were excavated from domestic house mounds and transported to the Peabody Museum at Harvard University where they are currently being analyzed. Both sites include materials dating from early Formative to Postclassic. A wide array of Maya ceramics is represented in the pottery from these two sites, so that the varieties and types present can be delineated with some reliability. These factors influenced us in selecting these collections for an initial application of the type-variety concept in the Maya subareas. A paper is being prepared that will discuss Smith's original report and designate

the specific varieties and types found in the Uaxactún collection. Similarly a monograph is in preparation that will contain the variety and type descriptions which pertain to the pottery derived from Barton Ramie. For present purposes we wish to outline here only the methodological approach that we have found most valid and useful.

Operationally, when dealing with the pottery from a single site such as Barton Ramie in a region previously unknown from a ceramic type-variety standpoint, one must first sort the material into ceramic units that are distinct from one another only because each represents a combination of attributes which, when observed together, is separable from some other combination of ceramic attributes. The term "ceramic unit" is applied to any ceramic entity (coalescence of attributes) about which the analyst has learned nothing beyond the fact that it is objectively sortable. A ceramic unit is an untested entity without known significance. These ceramic units remain units without taxonomic significance until the analysis advances knowledge concerning any one of them to a point where the variation encompassed by that unit becomes definable on the basis of a range in observable characteristics. At such time as the range of variation does become manifest and the ceramic unit seems meaningful, the various attributes combine as a potential ceramic variety. Under the rather special conditions that prevail in the Barton Ramie materials, where none of the varieties and types have previously been recognized, each provisional variety in the analysis is, in many instances, simultaneously a variety and a type due to the fact that an approximation of the total range of variation within the type, including time and space factors, remains as yet undefined.

As a result, types actually are best left undefined until the very final part of an analysis. When familiarity with its full range of variation is approached, a type usually comes to represent much more than any single vessel or small group of vessels. A type represents an aggregate of visually distinct ceramic attributes already objectified within one or (generally) several varieties that, when taken as a whole, are indicative of a particular class of pottery produced during a specific time interval within a specific region. Proceeding through a ceramic analysis at first leads the observer to the recognition of ceramic units; he is soon able to refine these into a number of potential varieties that in conclusion are used to delineate the ceramic varieties and types.

Limitations in the analyst's knowledge of the definable scope of any potential type as well as its distribution in time and space demand that all ceramic units be designated and tabulated on the variety level for purposes of preliminary analysis. In practice as well as in theory, then, when adhering to the type-variety concept as defined, all working ceramic analytical units are varieties from start to finish. The type is the more inclusive abstraction, and its definition as to content in terms of varieties should not be attempted until all the varieties *in the materials at hand* have themselves been distinguished and an idea of the ceramic materials from adjacent sites (in the case of Barton Ramie, sites such as Baking Pot, Benque Viejo, San José, Holmul) has given the student as great an insight as possible concerning the nature of varieties recognized earlier in the study. As previously noted, a type may be composed of only one variety, but whether it actually is or not is best tentatively decided as late in the analysis as possible.

The very nature of the type determines the validity of this fundamental principle. In the Phillips sense, the type is never an entirely stable entity; it is always in a relative state of flux, being added to or subtracted from as ceramic knowledge increases. When a type has been newly recognized in materials from a site, subsequent work in the same or nearby regions will often necessitate substantial additions or subtractions to the original description. But as information regarding a type grows so does the verbalization of its range of variation solidify to a point where changes in the main descriptive body are not pronounced even when the results of new field activities become known. Nevertheless, in a sense no analysis ever really completes a type definition, it only increases the correctness of the definition. And in any one analysis the most accurate picture of the types involved comes toward the end when the varieties all lie before the worker and he has familiarized himself as much as possible with the temporal and areal aspects of each on the basis of comparative materials as well as upon evidence within the collection itself. Toward the completion of his analysis, when the investigator does define types, he not only fulfills his duty to take steps in the synthesis of basic data but as far as the pottery at his disposal is con-

cerned, he consolidates his study and calls a necessary halt to the many analytical decisions that have plagued him in the classification of individual sherds.

This discussion has set forth the thinking behind our methodological approach. We view the ceramic variety as the basic unit of analysis, which in due course, consequent upon an increased depth of total ceramic knowledge, either becomes the type (as the established variety) or one of a number of varieties within the type.

Our consideration next focuses upon matters of nomenclature. If we follow the reasoning outlined above, what is the best way the ceramic units involved can be designated by name? In the interest of taxonomic consistency several observations are necessary and certain of our inclinations must be stated emphatically. The word inclination is purposely used rather than rule because our view can never be total, and what is best in one situation cannot be *ruled* best for a situation not yet observed. Our inclinations concerning nomenclature are as follows.

The binomial method of type naming widely employed in the taxonomies of North American pottery will be followed. Type and variety terms will be combined into single designations in the way Wheat, Gifford, and Wasley (1958: 37) have suggested; "Kiet Siel Polychrome: Awatovi Variety." Type names will be composed of a primary type term followed by a descriptive word that constitutes the secondary type term. Descriptive words may be hyphenated when necessary as in red-brown, incised-smudged, or neck-corrugated and in this way the binomial pattern is preserved by using these as single secondary type terms. Concerning the kind of primary type names to be used, we give decided preference to place or geographical names as is the practice in other territories of the New World.

Varieties are most successfully named in a manner similar to the naming of types, by assigning to each a different place or geographical name. In Kiet Siel Polychrome: Awatovi Variety, Kiet Siel (the primary type term) and Awatovi (the variety name) are both place names. Similarly in the Maya territory, Aguacate and Holha are both place names which we used in naming Aguacate Orange: Holha Variety. It has been observed, however, that in some regions of the Maya territory place names are few and far between. Also in certain instances analysts may feel the kind of technological or stylistic variation used to determine a variety must be brought out in the name. Accordingly some workers may wish to use a descriptive term derived from the variation involved, for example, Aguacate Orange: Thick-walled Variety.

There is a serious methodological consideration that favors the use of geographical names as variety names whenever possible. At the time a group of varieties is first described and named, the evidence for placing certain of them within the range of variation of one type or another is often inconclusive. Eventually enough evidence comes to the fore so that extremely accurate judgments can be made as to the proper positioning of varieties within a type. But in the meanwhile it sometimes becomes necessary to improve first assessments and a variety must be moved from one type to another. In its new type setting it may no longer be a "Thick-walled Variety" of the new type; it may be that all pottery in the new type is thick-walled and the variation in this different context is one of surface finish. Also by way of improving the accuracy of first assessments it often comes about that a ceramic entity originally classified as a variety within one type will, with the addition of new material, become quite distinct with respect to a majority of its attributes. Such a variety must be given separate type status of its own. In either case, when a descriptive name has been used to designate the original variety a change in type alignment or status may in the first instance and will in the second necessitate a new name. On the other hand, if a place or geographical name is used in the first naming, it stays tightly affixed to the ceramic unit wherever ultimately it may correctly come to rest (Fig. 1). Our disposition to leave type descriptions and assessments to the final stage of analysis and work through an initial sorting and interim sortings with ceramic units and variety units has previously been stated. It is frequently desirable to effect tabulations before the very final stage of any analysis. If each variety has been separately named with a different geographical name as it is recognized, sherds may be counted by their variety classification quickly and easily without waiting for clear indications of type affiliation to develop. No count is ever lost by using this method during the interim sorting because individually named varieties are the smallest meaningful units in a type-variety analysis.

Modes are easily referred to when necessary

ROARING CREEK RED
(TYPE)
ROARING CREEK RED: ROARING CREEK VARIETY
ROARING CREEK RED: MT. PLEASANT VARIETY
THIS TYPE-VARIETY ALIGNMENT COULD, WITH NEW EVIDENCE, BE ALTERED IN EITHER OF TWO WAYS:
ROARING CREEK RED
(TYPE)
VACA FALLS RED
(TYPE)
ROARING CREEK RED: ROARING CREEK VARIETY
(CHANGED TO A ONE-VARIETY TYPE)
VACA FALLS RED: VACA FALLS VARIETY
VACA FALLS RED: MT. PLEASANT VARIETY
1) A CHANGE IN TYPE ALIGNMENT (MOVED FROM ONE TYPE TO ANOTHER)
ROARING CREEK RED
(TYPE)
MT. PLEASANT RED
(TYPE)
ROARING CREEK RED: ROARING CREEK VARIETY
(CHANGED TO A ONE-VARIETY TYPE)
MT. PLEASANT RED: MT. PLEASANT VARIETY
(CHANGED TO A ONE-VARIETY TYPE)
2) A CHANGE IN STATUS (RECOGNIZED AS A SEPARATE TYPE)

FIG. 1. Diagram illustrating the methodological advantage in assigning a separate place or geographical name to each variety unit. If a descriptive name had been used to designate the Mt. Pleasant Variety in the initial instance, such a name might have had to be supplanted in change 1 and certainly would have been replaced in change 2.

by purely descriptive terms, such as "basal flange." If several distinctive forms of a special kind of mode are deemed of sufficient importance to identify separately, the practice followed in the southeastern United States adequately covers the situation. A suitable name is placed in quotation marks preceding the mode descriptive term. For example, the "Haynes Bluff" rim is a significant kind of rim form in the Lower Mississippi region of the Southeast. The use of a name or numerical code is preferable to a protracted string of adjectives or adjectival phrases that could easily assume extremely unwieldly proportions.

A sharp distinction is necessary between the basic working units of ceramic analysis on one hand and devices of a higher order of synthesis on the other. Among the latter may be included concepts such as "tradition," "ceramic system," "ceramic sequence," "ware," "ceramic complex," and "ceramic series." Each of these illuminates a particular aspect of the total ceramic scene and each is composed of pottery types. The basic working units of analysis are "types," "varieties," and "modes," any one of which may be described in terms of the attributes it embodies. Concerning these basic working units of analysis, no inference whatsoever must be implied by the name given a type or variety because these units *must* be maintained as free agents of analysis.

It has been demonstrated in other areas again and again that the types which one analyst finds present in a given complex, may well be proven by future researchers to be associated with different or new ceramic complexes. Not uncommonly evidence in a single site completely misleads excavators as to the chronological placement of a type. The total archaeological picture constantly changes in response to new information. We feel these considerations are powerful persuasions to the effect that a type or variety must *never* be restricted in its mobility due to its having been assigned the same primary name as a phase or as another type or variety. In the Southeast and Southwest often two varieties or two or more types share the same primary place name and as a consequence are identified by persons having no information to the contrary as being identical in their temporal and areal connotations. When these types or varieties were first described, this was believed to be true, but the results of many more site excavations have shown a number of these units to be quite dissimilar in temporal or areal extent. Exactly the same situation occurs when a phase and a type are given the same name. A pottery type, once an element of a ceramic complex within a large phase, may, if the large phase is split in two with the earlier retaining the original name, become a diagnostic of a complex within the later or new and differently named phase. If the original type bears the same name as the original phase (now the earlier) and in addition has the same primary name as another type which remains with the complex of the earlier phase, although retaining identical names, all must now separate and each represent differing time and areal connotations. Nevertheless, is not a reader unfamiliar with the details extremely likely and perhaps even entitled to assume a certain rela-

tionship when he runs at random across a type and a phase or two types designated with identical primary names? We think he is, and feel as a consequence the only way to guarantee that identical names will not linger and lead to misuse and unfounded inferences of a serious nature and to insure freedom of mobility for every type and variety is to use a different place name in every case, and refrain from using any name that has been employed previously as a phase name.

Phillips and Wheat, Gifford, Wasley are on record as favoring the use of the same variety name when indicating the same variation as it obtains with regard to two basically different types. The present authors have reconsidered the matter in consultation with Phillips and believe the arguments enumerated above outweigh any of the original judgments. Accordingly every variety and therefore *every type* discerned in the materials from Uaxactún and Barton Ramie has, without exception, been designated with a different place name and none of these has been used as a phase name. We firmly stand by this inclination even though in the purest sense we realize the truth in and fully subscribe to Colton's view that as far as a name itself is concerned, a place name or any other primary name used does not on its own determine the time element involved or the "area of greatest density, for the name simply serves as a label for reference" (Colton 1953: 53).

To summarize our inclinations concerning nomenclature:

1. Primary type terms should be place or geographical names (San Antonio).

2. Secondary type terms should be appropriate descriptive words hyphenated when necessary (San Antonio Golden-brown).

3. Variety names should preferably be place or geographical names, but may be descriptive terms if such names are necessary or desirable (Hillbank Red: Hillbank Variety — an established variety; Aguacate Orange: Holha Variety — a place name variety; Aguacate Orange: Thick-walled Variety — a descriptive name variety).

4. No two types or varieties should ever share the same place name as a taxonomic label. In other words, once a geographical name has been assigned as a type or variety name it should never be affixed to any other type or variety (the single deviation from this pattern would occur when the established variety of a type has the same place name as its type).

5. No phase name should ever be used as a type or variety name.

6. Types and varieties are the basic working units of any ceramic analysis formulated on the type-variety concept and to keep the nomenclature from interfering in any way with the freedom and mobility of these fundamental entities is a paramount consideration.

Since the variety is *the* sorting unit in every type-variety analysis, all names are initially applied to varieties. In Phillips's scheme, the first variety described is the entire type content until more varieties of the type are observed. Therefore the first identified variety of a type becomes the "established variety" and lends its name to the type because they were initially synonymous. Because these Maya collections are the first to be subjected to type-variety analysis, many of the varieties are alone in representing their types; when these types are described, each variety automatically becomes an established variety, thereby furnishing the type names. In cases where more than one variety constitutes a type, none of the entities having been previously named in print, one of the several involved is arbitrarily designated the established variety and in this capacity lends its name to the type. Having classified and named all ceramic units in this way, the alignment of types into larger categories such as wares or ceramic systems can be effected and re-effected in accord with new information without restrictive connotations or residual overtones due to prior affiliation.

We may have made it appear as though types and varieties come and go and move about every time someone picks up a sherd. This, of course, is not our intention. Our emphasis upon change in the alignment of ceramic units has to do with our concern for pottery analysis in the Maya subareas over a good many years to come; we are interested in embarking upon a taxonomic scheme that has some promise of being as useful to tomorrow's analyst as it may be to us. There is a great deal of value, we believe, in attempts to make data as useful to subsequent workers as is conceivably possible. The more ceramic evidence comes to light, the more stable each type and variety becomes. Eventually, as the ceramic complex peculiar to every phase develops and is more closely defined, the ceramic inventory for various archaeological cultures comes closer and closer to a true approxi-

mation of ceramic configurations as they once existed. As archaeological research proceeds along these lines, the types and varieties become more and more correctly posited and their proper relationships become a great deal more firm in the eyes of different workers. The important point is, however, that the taxonomy must not preclude change, now or later. It must allow for revaluation, revision, and subsequent shifting of ceramic units.

As the analysis of our collection progresses from the washing screens to the final type descriptions, the application of the taxonomic method under discussion proceeds along certain lines. These are most conveniently described in terms of three sherd sortings: initial, interim, and final. The interim sorting refers to all those rearrangements that occur on the sherd tables between the initial breakdown and the final sorting from which finished tabulations are made and type descriptions drawn.

Every distinguishable ceramic unit or potential variety that proves significant is a variety and each must be named separately. Although the primary type names are assigned only at the conclusion of the final sorting, the class to which a potential variety belongs is evident after the initial sorting and it is already quite apparent whether the potential variety will be a polychrome, a red-on-black, or a red, and so on. Accordingly the secondary term of the type name may be assigned at the outset along with the variety name. To demonstrate the entire procedure, let us consider a group of nine ceramic units.

Immediately after the initial sorting a secondary type term and a potential variety name may be assigned each of our nine ceramic units (assuming, of course, that every unit is indeed recognizably distinct from each of the others). We will have distinguished, then (1) Red: Hillbank; (2) Red: Rockdondo; (3) Red: Starkey; (4) Red: Vaquero Creek; (5) Orange-red: Macal; (6) Orange: Aguacate; (7) Black-on-orange: Gavilan; (8) Polychrome: Coquericott; (9) Incised: Macaw Bank; (Fig. 2). When an attribute appears to be particularly distinct it is important to make note of it. This is accomplished by placing the observed feature in parenthesis after the variety name. This signifies

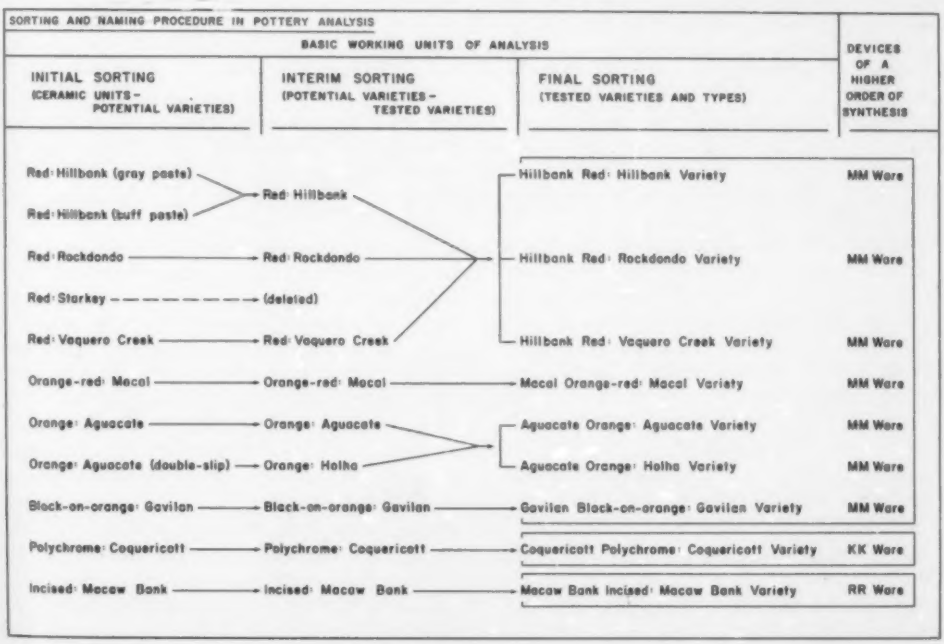

FIG. 2. Diagrammatic representation of the process of sorting and naming varieties and types in a ceramic analysis.

that if combined with other features, the attribute could assume significant importance and be the means whereby a distinguishable variety might be recognized. Examples are provided in the case of Red: Hillbank, and Orange: Aguacate. In Red: Hillbank, two different colors are observed in the paste, gray and buff; in Orange: Aguacate, some sherds seem to show a double slip.

As the interim sorting gets farther along our knowledge concerning ceramic unit range of variation and the validity of first judgments increases. It becomes apparent that the recognition of Red: Starkey was based upon features that fall within the ranges of variation now known to be encompassed by Red: Rockdondo and Red: Vaquero Creek. Red: Starkey is therefore abandoned as a ceramic unit and the sherds attributed to it resorted between Red: Rockdondo and Red: Vaquero Creek. As to Red: Hillbank, it was found that a range in paste color from gray to buff may occur in a single vessel and is due to variation in firing temperatures. As a result there is no reason to continue any separation based on this attribute and Red: Hillbank (gray paste) and Red: Hillbank (buff paste) are combined into Red: Hillbank alone. Orange: Aguacate (double-slip), on the other hand, has become more distinct until it is now clearly distinguishable as a variety. It is therefore designated by a new term, Orange: Holha. The other units become more easily recognizable and the range of variation within each increases but they remain as originally named because the significant criteria involved have not altered.

After the final sorting has taken place and as much as possible has been done by the analyst to familiarize himself with materials from adjacent and neighboring sites, he is in a position to judge the nature of the types present and has an idea of time and space values involved. These estimates are more or less final in terms of the analysis at hand and the resulting types are those described in his report. It has become plain that technological and stylistic relationships between Red: Hillbank; Red: Rockdondo; and Red: Vaquero Creek are quite close. In addition only inconclusive time differentials seem to exist, while in geographical distribution there appears to be a general coincidence. These are, then, three varieties within the same type. Because none of these varieites has been described in print before, the one which is to be the established variety must be chosen arbitrarily from among the three. Red: Hillbank is the most easily recognized so it is selected to be the established variety. If one of these units had previously been described in published form by another authority, it would be the established variety. The name Hillbank, derived from Red: Hillbank, then becomes the type name and we have Hillbank Red as the type, to give us Hillbank Red: Hillbank Variety; Hillbank Red: Rockdondo Variety; and Hillbank Red: Vaquero Creek Variety. Similarly Orange: Aguacate and Orange: Holha are varieties of one type. Orange: Aguacate is chosen as the established variety and we have Aguacate Orange: Aguacate Variety, and Aguacate Orange: Holha Variety. Orange-red: Macal and the other ceramic units remain distinct and no other varieties seem related to them on the basis of this analysis. Each of these is therefore an established variety that for present purposes alone constitutes the type. As a result we have Macal Orange-red: Macal Variety; Gavilan Black-on-orange: Gavilan Variety; Coquericott Polychrome: Coquericott Variety; and Macaw Bank Incised: Macaw Bank Variety. It is evident that to avoid confusion *the word variety must at all times accompany the variety name.* Hillbank, for example, standing by itself could be construed either as the type or the established variety, but if Hillbank is always accompanied by either the term Red or the word Variety, there can be absolutely no doubt that the former (Hillbank Red) refers to the type and the latter (Hillbank Variety) refers to the established variety.

Finally the analyst may present these basic ceramic units in arrangements that have significant interpretive value in whatever manner the evidence at his command may seem to dictate. None of these arrangements will ever destroy or bias the original variety units because each variety remains as described from the data and can be shifted into any new alignment or theoretical position regardless of the names involved in any of the new configurations. Seven of the nine varieties can effectively be placed within a single ware (MM Ware) without prejudicing future arrangements (Fig. 2). Following the methods outlined, Vaquero Creek Variety could be extracted from Hillbank Red Type as well as from MM Ware and placed within a new type under another ware, if new evidence made this necessary, without the

slightest taxonomic confusion or misconception due to relationships implied by any name involved. Similarly, even though it is a variety within MM Ware, Gavilan Black-on-orange: Gavilan Variety can be placed as an element within a completely different theoretical device such as a particular Black-on-orange ceramic system with implications and ramifications that are entirely new. No taxonomic difficulties whatsoever are encountered in these variety reassignments. Complete freedom and mobility has been maintained for the basic units of ceramic analysis by a utilization of the type-variety concept and by rigidly adhering to a fundamental procedure of nomenclature.

On the face of it, and during the first applications of this method, there may be those who are inclined to consider a large number of varieties, each with a different name, as a disadvantage. But it will soon be observed that by allowing all the variety units to be distinguished and named individually, the number of named type units is not in any way erroneously increased (in fact this method tends to prevent the erroneous multiplication of named *types*). Types are the units most clearly reflecting cultural phenomena; these are the units of cross-cultural comparison, the units of synthesis, the units that are of greatest use to interested parties in general (teachers and researchers alike). Types are most likely to be committed to memory as the elements diagnostic of a ceramic complex within a phase. The varieties are, by contrast, the units of basic laboratory analysis. Even though they are meaningful units, they are tools of the specialist; they are the fine breakdowns necessary at a certain level of research analysis. But, and this is most important, it is not at all necessary for all workers or for persons in allied fields to have them at tongue tip or even be familiar with them. Varieties are of maximum use to the specialist. As a consequence there should be no limitations on their number or on the proliferation of variety names on this level of analysis. When a ceramic unit becomes recognizable as a "ceramic variety," it must be properly designated. Those who prefer to think, work, and teach in terms of larger ceramic units may restrict their activities to the level of pottery types without doing violence to any aspect of the methodology.

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THE TYPE-VARIETY METHOD OF CERAMIC CLASSIFICATION AS AN INDICATOR OF CULTURAL PHENOMENA*

JAMES C. GIFFORD

ABSTRACT

This discussion deals with the theoretical reasoning that underlies the type-variety method of ceramic analysis. Not only do pottery "types" and "varieties" embody sets of recognizably distinct attributes and impart particular cultural, areal, and temporal connotations, but they are also meaningful entities of cultural interpretation.

1. "Varieties" that have been tested by continued close study and by increases in knowledge concerning their nature and range of variation are close approximations and reliable indicators of original ceramic manifestations due to individual or small social group variation in a society.

2. Ceramic "types" represent the combining of a number of attributes into abstract conceptions which, when executed in clay by potters, are acceptable to them and a majority of others within their cultural configuration. Types are summations of individual or small social group variation consistent with boundaries imposed by the interaction of individuals on a societal level and determined by the operative value system present in any society. Pottery types are therefore representative of cultural phenomena.

3. Ceramic types are cultural derivatives and can be related one to another through space and time. Relationships of this kind are recognized as "ceramic systems" and "ceramic sequences."

ONE OF THE primary reasons why the archaeologist finds it advantageous to analyze pottery in terms of typological concepts is that a pottery type, once recognized and described, will inevitably repay the analyst by providing him with certain information otherwise unavailable or only available in part. Over thirty years ago Vaillant (1930: 9) observed "that the backbone of most of the New World chronologies is variation in pottery types and that the arrangement of a tribal ceramic into chronological divisions is not only very technical but also highly interpretive and impressionistic." The minimum elemental information now held to be usually bound up in a type is that the ceramic entity, such as Benque Viejo Polychrome, a type, is a specific kind of pottery embodying a unique combination of recognizably distinct attributes, and that this pottery, as well as being the product of a certain cultural configuration, has a definable areal distribution and temporal significance. When the analyst has set down a pottery type description, then the taxonomic device, or pottery type name, he attaches to the described ceramic entity, comes to stand for the entire set of recognizably distinct attributes plus a particular cultural affiliation, areal distribution, and temporal connotation.

In addition anthropological schemes of classification such as this should be means to define and weigh the relative importance of variation and regularity in human society. Most classifications used in anthropology find their rationale in that part of the scientific method which subscribes to the basic assumption that out of what may appear to be a mass of variation, regularities or laws of (cultural) process may be discerned, delineated, and described.

A scheme of ceramic classification has been proposed in three recent papers that is geared especially to the analysis of material aspects of human culture and that also provides a foundation upon which theoretical cultural considerations can be based. These three papers outline and define what is called the "type-variety concept" (variety, Wheat, Gifford, and Wasley 1958; type, Phillips 1958; type-variety concept, Smith, Willey, and Gifford 1960). In the following discussion whenever the term "type" is used, it is intended strictly as defined by Phillips. His usage corresponds to the "type cluster" in the Wheat-Gifford-Wasley nomenclature. For present purposes the terms "type," "variety," and "type-variety concept" are words with a special meaning not to be construed in settings other than those set forth in the three interpretations cited.

Within the context of the type-variety concept, variation as a recognizable reality can be thought of as the product of the individual or of relatively small social groups in human society. "Individual and small social group variation" is meant to encompass not only (at a minimum) actual individual variation, but also variation due to the activities of potters in a village segment, a village, a group of small villages, a community, or a series of communities (at a maximum). When entire cultural configurations are taken into account certain regu-

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larities are discernible that are due to the interaction of individuals and small social groups within a society, and these are observed as types. Types in this sense are material manifestations of the regularities of human behavior. Furthermore, theoretical conceptions such as "horizon style" (Willey 1945), "tradition" (Thompson 1956; Willey and Phillips 1958: 34–40), "ceramic system" and "ceramic sequence" (Wheat, Gifford, and Wasley 1958) are based on the assumption that on higher levels of societal integration, currents may be discerned which represent the very generalized responses of human groups as determined by recurrent situations and pressures of an even higher order of regularity in human behavior (Fig. 1). The type-variety concept accepts the premise that "cultural phenomena, by definition, have other than a chance distribution" (Kluckhohn 1958b: 40) and that there is discoverable order in the data. If the behavioral regularities of societies are documented by means of ceramic and other schemes of classification, some of the laws of human behavior which may exist will become apparent. Classificatory schemes applied to archaeological materials are in part useful as a means toward this end.

These views come to center in particular about the idea that the "type" as an entity is meaningful from the standpoint of cultural interpretation. In the most general terms and to varying degrees, this sentiment is adhered to by many others who have conducted and written about ceramic analyses. Nevertheless, there are competent analysts who do not concur with this view; they believe it tends too far in the direction of interpretive inference. Most previous discussions of ceramic data, however, have been severely limited because the type was the only accepted unit of analysis. Limitations have also come about through an insufficiently coordinated use of theoretical conceptions having to do with other factors in the cultural context under investigation.

The possession of a more flexible definition of the type in conjunction with the variety as well as other ideas such as those embodied in the "ceramic system" and the "ceramic sequence," makes it possible to add new dimensions to a discussion which previously of necessity had to revolve about notions restricted to ceramic units of equivalent status. Within the type-variety concept analysts do not need to refer to all entities as types, and the newer levels of analytical deliberation introduced by an application of more than one theoretical conception upon the ceramic material open up avenues of greater freedom in our thinking. It may thus be possible to bring under one roof viewpoints and opinions previously thought to incline toward opposite poles and be irreconcilable.

When conducting a ceramic analysis utilizing the type-variety concept, the ceramic units involved in the interim sortings are designated "potential varieties." These potential variety units are useful because they are separable on the basis of attributes which are recognizably distinct from one another, but at this stage in classification, these units may or may not be contrivances or artifical constructs of the student. Nevertheless, as any pottery analysis proceeds and as other studies are conducted involving like materials, knowledge concerning each variety increases to the extent that those varieties which *are* only artificial constructs can be discarded on the basis that they have no cultural reality and therefore virtually no further utility. Varieties which survive each addition to knowledge concerning them more and more closely approximate actual material ceramic manifestations of individual and small social group variation in a society. Consequently one may admit that some preliminary variety units indeed prove to be creations of the worker, but in accord with the underlying premise that classification for classification's sake alone is not justifiable, those variety units which expanded knowledge does reveal as having been without cultural meaning are discarded. The more we know of varieties and types, the better we are able to formulate them into units that approximate what were meaningful entities in whatever cultural context we are endeavoring to illuminate.

Types generally include several or many varieties, and as a result are summations of individual and small social group variation. The basic attributes involved in any type came together in the combination of a mental image plus the motor habits of the prehistoric artisans of a culture in such a way that when executed in clay, they fulfilled the requirements of the ceramic and stylistic values of that culture. As noted by Kluckhohn (1958a: 474): "In the last analysis it is clearly from individual variability that new cultural values take their origin." And so it is that "types," having grown

out of a blending of individual variation, both reflect cultural values and are determined by them. A type is regarded as being the material outcome of a set of fundamental attributes that coalesced, consciously or unconsciously, as a ceramic idea or "esthetic ideal" — the boundaries of which were imposed through the value system operative in the society by virtue of individual interaction on a societal level. These ceramic ideas occurred in the brains of the potters who made the ceramic fabric that constitutes a type, and they are not by any means creations of an analyst.

The distinction of one variety from another within a type rests upon differences having to do with one or several minor characteristics. In contrast with those particulars which distinguish one type from another, such minor ceramic differences were the result of work produced within the confines of relatively small social groups or by individual potters who indulged preferences as to the locale where temper or clay must be gathered or who were able to give vent to artistic flairs and so on. As such, varieties cannot be held as representing any cultural configuration in its entirety. Varieties are apt to reflect "individual and small social group variation" rather than whole-culture phenomena, while the type portrays a combination of a number of pottery traits that were acceptable not only to the potter but to most others adhering to a given culture pattern (Fig. 1). Here we approach certain important observational points that bear upon the reasons why types do represent cultural phenomena.

> There are psychological causes or mental conditions — generally considered physiological — which might also be called "tendencies." Such are the tendency to fatigue, the tendency to form habits, the tendency toward imitation by suggestion, and others. These exist nearly identically in all men, whatever their degree of civilization. . . . The tendencies of which we have spoken are at the root of all anthropological phenomena (Kroeber 1952: 18).

The "tendency to form habits" and "the tendency toward imitation by suggestion" are intimately connected with the basic causal factors intertwined in the cultural validity of the type.

In the words of a passage contained in a theoretically oriented study by Rands and Riley can be found a pertinent statement of this validity. Cultural phenomena and processes can be documented in the identification of varieties and types because "culture growth often involves a great deal of elaboration — those embellishments which so often impress the student by their number and variety. At the same time, habit channeling, together with the nature of the complex nucleus, will tend significantly to delimit the forms of embellishment that are likely to take place" (Rands and Riley 1958: 276). Applying the first of these remarks to prehistoric pottery, the "ceramic variety" represents elaboration and embellishment, whereas the second statement refers to that which results in a combination of abstracted elements that in pottery is represented by the "ceramic type."

It is plain that a pottery type has a cultural basis because the implicit values inherent in the cultural configuration as a whole cast serious limitations upon what in the realm of ceramic products will be acceptable to the participants in that cultural configuration. At the same time the cluster of values also imposes serious strictures upon the ceramic mental images that the potters will draw upon. The potters respond to value-sanctioned, culturally-defined images and not to others that could be encompassed by their abilities or imagination because most people, due to economic necessity and for reasons of greater psychological comfort having to do with reduction of social stress, tend to conform to the demands of a majority of the norms that are a part of their culture at a particular time in history. "To speak of 'values' is one way of saying that human behavior is neither random nor solely 'instinctual' . . . the individual gets abstract, perduring standards primarily from the culture or sub-culture as mediated by parents and other persons from whom he learns" (Kluckhohn 1958a: 474). Types, therefore, equate themselves with the crystallization of conscious or unconscious ceramic esthetic images conditioned by values. In this regard, the scheme of classification discussed approaches a definition of the ceramic value system which pertains to any given archaeological culture. In other words, when recognizing and defining types, the analyst is describing the material manifestations "of preferred paths of behavior that take their direction from varying concepts of the desirable" (Kluckhohn 1958a: 473).

In speaking of artifacts Leslie White says, "We do not customarily call these things human behavior, but they are the embodiments of human behavior; the difference between a nodule of flint and a stone axe is the factor of human labor. An axe, bowl, crucifix — or a haircut — is congealed human labor." White maintains

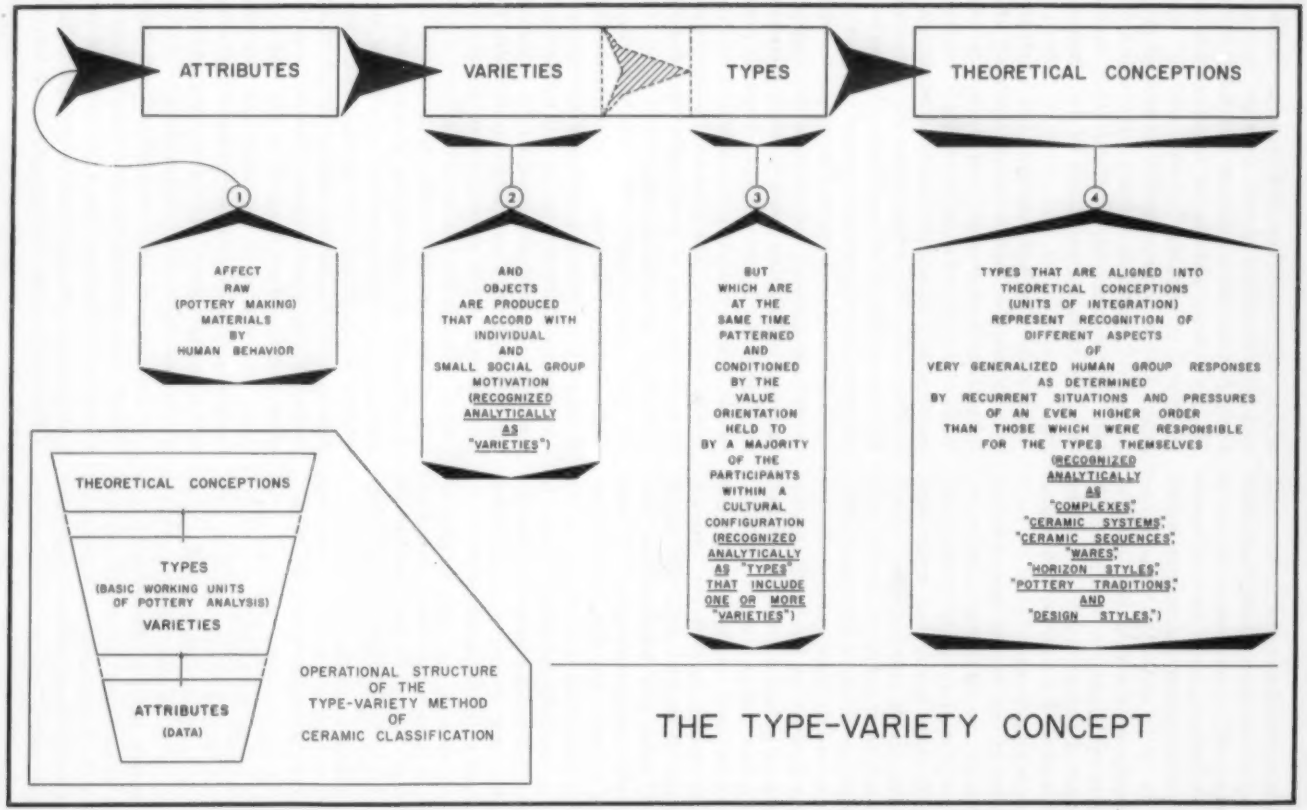
ATTRIBUTES
VARIETIES
TYPES
THEORETICAL CONCEPTIONS
1
AFFECT RAW (POTTERY MAKING) MATERIALS BY HUMAN BEHAVIOR
2
AND OBJECTS ARE PRODUCED THAT ACCORD WITH INDIVIDUAL AND SMALL SOCIAL GROUP MOTIVATION (RECOGNIZED ANALYTICALLY AS "VARIETIES")
3
BUT WHICH ARE AT THE SAME TIME PATTERNED AND CONDITIONED BY THE VALUE ORIENTATION HELD TO BY A MAJORITY OF THE PARTICIPANTS WITHIN A CULTURAL CONFIGURATION (RECOGNIZED ANALYTICALLY AS "TYPES" THAT INCLUDE ONE OR MORE "VARIETIES")
4
TYPES THAT ARE ALIGNED INTO THEORETICAL CONCEPTIONS (UNITS OF INTEGRATION) REPRESENT RECOGNITION OF DIFFERENT ASPECTS OF VERY GENERALIZED HUMAN GROUP RESPONSES AS DETERMINED BY RECURRENT SITUATIONS AND PRESSURES OF AN EVEN HIGHER ORDER THAN THOSE WHICH WERE RESPONSIBLE FOR THE TYPES THEMSELVES (RECOGNIZED ANALYTICALLY AS "COMPLEXES," "CERAMIC SYSTEMS," "CERAMIC SEQUENCES," "WARES," "HORIZON STYLES," "POTTERY TRADITIONS," AND "DESIGN STYLES,")
THEORETICAL CONCEPTIONS
TYPES
(BASIC WORKING UNITS OF POTTERY ANALYSIS)
VARIETIES
ATTRIBUTES
(DATA)
OPERATIONAL STRUCTURE OF THE TYPE-VARIETY METHOD OF CERAMIC CLASSIFICATION
THE TYPE-VARIETY CONCEPT

FIG. 1. Chart showing how the analyst can recognize certain cultural phenomena in a collection of pottery by applying the type-variety method of ceramic classification. (In terms of absolute theory: within a cultural matrix where the individual motivation of all participant pottery makers coincides completely with the ceramic and stylistic values of that culture, all types would tend to be exclusively one-variety types with a certain range of variation in each case as determined by participant diversity in skills and so forth; on the other hand, within a cultural matrix where no value orientation obtains concerning pottery or where the value orientation has for some reason been totally shattered, the products of every pottery-producing individual would be a distinct variety and the number of varieties within a type would be limited only by the number of pottery-producing individuals within the cultural configuration.)

artifacts are the result of acts which cumulatively, in this sense, are human behavior and the concern of psychology. "If, however, we treat them [artifacts] in terms of their relationship to one another, quite apart from their relationship to human organisms, i.e., in an extra somatic, or extraorganismic, context, the things and events become *culture* — cultural elements or culture traits." The difference between the two "derives from the difference between contexts in which their common subject matter is treated" (White 1959: 232–3).

Tacit recognition is accorded these well-stated contextual observations in the implications of the type-variety concept. The situation, however, is treated not as a dichotomy but as a duality (one flowing from the other) in that the "variety" is a reflection of concrete individual human behavior while a "type" represents an abstraction from individual or small social group behavior that is the ceramic unit most useful in showing relationships of one kind of pottery (one ceramic cultural element) to another. Furthermore when viewed in this light the type-variety concept tends to be consonant with the definition of culture by Kroeber and Kluckhohn (1952: 155) who say it "is an abstraction from concrete human behavior, but it is not itself behavior." By analyzing pottery within the framework of the type-variety methodology we move from a consideration of human acts to the consideration of theoretical abstractions that can be used in various ways to increase our understanding of cultural processes (Fig. 1). It is an analytical progression from the specific to the general, from percepts to concepts, and in this case from appreciable examples of human behavior (in pottery making) to concern with abstract cultural elements.

In the light of the foregoing remarks as they pertain to innate human tendencies, the archaeologist, in his capacity as a student of material culture, seems to come more directly into contact with what may be subsumed under two particularly fundamental tendencies. These two tendencies are to be found as components of elemental human nature in the form of (1) stability, as expressed by type persistence and continuity through time, a condition essentially embodied in the concept of tradition (Thompson 1956; Willey and Phillips 1958: 34–40); and (2) change, as encompassed by the constant regrouping of artifact attributes into new types. In each individual craftsman there is a rudimentary desire for a measure of both stability or continuity (manifested by habits and a clinging to the old) and change (manifested by the desire for something new). The extent to which either of these is satisfied or emphasized is conditioned by the total value system of the society in which the individual finds himself. Some societies emphasize fast moving change accelerated to such a point as to be constantly disturbing to the individual. Others emphasize stability to such a degree as to be almost dormant. On his own the individual seems to incline toward a fine balance of change and stability, and his desires would be met under ideal conditions by a rhythmic pattern of new types within established traditions. Actual situations deviate from the ideal in accord with the degree to which stability or change is emphasized within a cultural matrix. In trying to describe and observe the tendency in any given configuration toward one or the other extreme we approach an understanding of the underlying mechanisms of human nature and the societal values that produce types as observable realities. In describing types and recording the material traditions within a society we are indicating the tempo of that society as reflected in its material culture.

Cultural anthropology has consistently drawn attention to tendencies of this kind and there has been an effort to delineate them with regard to value orientations. A recent paper (Colby 1958: 317–22) pinpoints the problem in the context of an equilibrium theory of behavioral redundancy. In any cultural configuration

> predictability of tomorrow's events and of what people may do or think is of paramount importance. . . . For predictability to be constantly maintained at a high level of efficiency, value systems must be changeable, must always be in process. . . . But there are other trends in the opposite direction which temper the dread of the unexpected. These are trends toward disorganization, toward the unknown, and toward experimental innovations. . . . By maintaining some sort of equilibrium between order and disorder or rigidity and fluidity [man] can stay at peak adjustment. Such is the maturation process in culture and in personality. . . . One may say then that life in general, like language, seeks . . . an equilibrium between the new (unexpected) and the old (predictable); between disorganization and organization.

In consequence of these underlying tendencies of human behavior as discussed by Colby, there is in any cultural continuum a "process of constant reformulation, reassessment, and affirmation of value structure regulated by the movement either toward or away from organization."

It is this interesting aspect of human nature that provides a considerable explanation of the "why" and the cause of dynamics in cultural process. The archaeologist sees and documents it when he describes gradual change within a cultural or artifact "tradition." The ceramic analyst has observed similar circumstances when he elucidates a "ceramic sequence."

The fact cannot be overemphasized that it is due to the results of the combined value forces of the total cultural configuration (indeed the inherent cultural bias) that particular kinds (types) of pottery become widely acceptable and are striven for as desired norms in ceramic manufacture. Awareness must be exercised that any normal "human act, even in its first expression in the person of a single individual, is a group product to begin with" (White 1959: 244). The individual craftsman is never really "free" to "create" the entity recognized as a variety or type because although he may be free to produce an entity of some sort, his production may either be accepted or rejected by the culture in response to its particular bias. And since the individual did not by himself create the bias, the cultural acceptance of his product is beyond his immediate control. The results of his work may, in terms of the culture, merely go down as experiment, thereby resulting only in one or a few expressions of individual variation, or it may "catch on," that is, be compatible with the bias of a major portion of the culture and thus have an appeal which is pleasing or advantageous in one way or another to a majority of the participants in the culture. And of course, due to the mechanisms of culture change through time, the cultural bias shifts with time and what will please at one time may not please at another — so it is that a type is a true indicator of time and a complex of types (a ceramic complex) is a valid delineation of the ceramic content of a phase.

To repeat the substance of many an earlier discourse, a potsherd is surely not a biological entity and therefore cannot in a biological sense be related to or evolve into another kind of potsherd. But let me reiterate my view that pottery types are manifestations of ceramic ideas, ceramic images, held originally in the minds of human beings. The ideas are human concepts and as such can be and are indeed related to one another areally and do indeed, as ideas or conceptions develop one into another through time. Relationships of this order are recognized by the analyst through the medium of theoretical conceptions such as "ceramic systems" and "ceramic sequences" (ceramic micro-traditions).

Ideas, mental images, are the "genes" of culture. These ideas or mental images, and in particular ceramic ideas or mental images, while being in no way biologically related to other mental images, can be and often are related within cultures and cross-culturally within a cultural frame of reference (see Osgood 1951: 202–14). Therefore, anthropological classifications having to do with ideas or mental images as recognized in objects of material culture find their basis and justification within the processes of culture itself. Such classifications as these should not in any sense be viewed as biological classifications. They are cultural classifications. As a result, even though man is a biological organism, the "type-variety" methodological approach is a cultural classification because it is concerned with the analysis of man's nonbiological *culture products*.

An interesting point has been raised by Kluckhohn to the effect that classifications which are to be of use to anthropology should be typologies. He defines a typology as

> a classification that has an intent, that is, has a direction. The ways one may classify things are limitless and therefore any number of classifications may be conceived, but a typology has an explicit theoretical basis and the typologist is interested in using a given classification in order to shed light upon the reasons beneath the occurrence of some observable phenomena. . . . A classification is no more than a set (or sets) of empirical groupings established for convenience. A typology, however, is a theoretically oriented classification that is directed toward the solution of some problem or problems (Kluckhohn, in press).

Following this distinction I view the type-variety methodological scheme as a typology. In addition, when an analyst subjects a collection to study by means of the type-variety concept, I believe he accomplishes both what Rouse (1960) calls "analytic classification" and "taxonomic classification."

In conclusion, let us review the query, should an anthropological typology be an end in itself? Absolutely not. Any classification of use to anthropology, whatever the medium, must be but a means whereby cultural processes are described and elucidated. Within such a frame of reference cultural classifications (anthropological typologies) are some of the instruments which anthropologists must use if they are to

chart reliably the actualized and potential dimensions of human nature through time and space.

This is not by any means to say that it is desirable to adopt blindly any one scheme and stick to it come what may. It is rather my feeling that ultimately a classificatory scheme or group of compatible schemes will be refined which will accurately reflect the regularities and irregularities, the similarities and dissimilarities, of cultural processes, perhaps on a world-wide basis, and that our present efforts are but steps in this direction.

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UNDERWATER ARCHAEOLOGY: ITS NATURE AND LIMITATIONS *

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ABSTRACT

The recent development of efficient free-diving apparatus has made possible underwater exploration for both sport and science. Human and cultural remains underwater are being extensively exploited. Although the term "underwater archaeology" is frequently applied to such activities, most are at the best only underwater salvage. Archaeological remains in lakes, springs, rivers, or the sea have considerable to offer. Perhaps their greatest value is the more complete condition of such artifacts in contrast to similar remains on land. However, they often suffer from decomposition, impregnation with minerals, or incrustation by algae or corals. Although context and sequential (or stratigraphic) position are not so easily found or recognized as on land, they are present in many cases. Any body of water is a possible site of study, but in practice, areas of potential importance are limited by location, depth, visibility, and bottom factors. There are four major types of underwater archaeological sites: (1) discarded refuse and "lost" material; (2) submerged sites of former human occupation; (3) shrines or places of offerings; and (4) shipwrecks.

WITHIN THE LAST 20 YEARS the development of three devices, the glass face mask, rubber foot fins, and a practical self-contained underwater breathing apparatus (SCUBA), has opened up the shallow underwater world for man's free exploration. This broad new area for discovery has been a fascinating zone for man from Classical times to the present. The first successful (not theoretical) efforts of man to operate undersea, other than by surface diving, were accomplished apparently in the 16th century, by means of a diving bell. This diving bell, of course, gave a diver some depth of action, but limited him to a radius of free swimming away from the bell to the extent of air in his lungs. Next, early in the 19th century, came the first primitive diving suit. However, no matter how efficient this mechanism became, a diver was limited in movement by the clumsiness of the gear and the attached air line. The first equipment to allow man time and more free movement undersea were various self-contained oxygen rebreathing apparatus developed after 1911. These, nevertheless, had a major disadvantage — their danger below about 25 feet, where pure oxygen taken under pressure brings on toxic poisoning.

True free diving with effortless movement (a result of foot fins), and clear visibility (with a face mask) were achieved in France by 1938. The last most important element, an adequate, self-contained, safe compressed-air supply, was attained in 1943 by the development in France of the "Aqua-Lung" by Jacques Ives Cousteau and Emile Gagnon. This invention climaxes the fascinating history of man's struggle to successfully survive and operate undersea from antiquity to modern times, which is so well described by Latil and Rivoire (1956). Subsequent development of the "Aqua-Lung" prototype by its inventors and variations developed by others have given man a device which, with certain limitations of time and depth, make him free and mobile undersea. While the use of these devices has become a world-wide sport, their application to serious problems involving biology, geology, history, and archaeology has become equally significant.

This discussion of certain aspects in the field of underwater archaeology is based on work which began ten years ago with the discovery of the Fig Springs site in Florida. Most of the illustrative examples in this paper have been chosen from the New World on the basis of my own experience and the interest of American readers.

It should be noted, though, that New World activity in this area cannot compare in volume or scope with the work carried out in the Mediterranean. Some general accounts of this work have been publicized in well known journals. As a result one is more impressed with the diving techniques and the artifacts found rather than with the archaeological results. Apparently Philippe Diolé (1952), a well known French journalist, was the first to write a "book" on underwater archaeology. However, most of the popular works on diving have included chapters on this subject. The best of these is probably Dugan (1956, Chap. 10); but Latil and Rivoire (1956, Chaps. 16–17) and Doukan (1957, Chap. 5) can also be mentioned. For the field of shipwrecks and "treasure trove" the best organized, least emotional, and most prac-

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tical book, in terms of every day problems of diving, is that by Marion Link (1959). Brief or preliminary notes on specific Old World sites or areas are surprisingly few, but we can mention Casson (1953), Cousteau (1954), and Demangel (1950).

Coincident with the development of underwater exploration there has appeared considerable literature on "underwater archaeology" written by diving enthusiasts which has captured public interest. Some reports are unpretentious, others attempt a more formal treatment. Few achieve what we consider basic coverage in archaeology, that is, the presentation of field methods, adequate description of artifacts both quantitatively and qualitatively, a full discussion of their context, and the drawing of adequate conclusions. As illustrative of this type of article see *Skin Diving* (Lynwood, Calif.), a magazine now in its eighth volume and the best in its field.

Where archaeologists have participated in the picture, the results are better. The work of Stephan F. Borhegyi at Lake Amatitlán, Guatemala, is a good illustration, although Borhegyi did not dive. His reports are only preliminary, but he appears to cover his subject adequately (Borhegyi 1958, 1959). The artifacts are described, and, while numbers are not given, he apparently has them. Most importantly, the different contexts in which the specimens were found are discussed.

Far more difficult to evaluate are the accounts of "underwater archaeology" which have been sponsored by reputable institutions or journals. These are a matter of much more concern because of their "professional aura," although an examination of their techniques reveals them to be unacceptable to professional archaeologists. Two of these can be briefly discussed.

The first, *The Last Cruise of H. M. S. Loo* by Mendel L. Peterson (1955) is an account of diving on a wreck off Looe Reef in the Florida Keys. A few artifacts are mentioned in the text with some illustrations, but no adequate description of them is given and, above all, no counts or even approximations of absolute or relative quantities of these specimens. It may be argued that artifacts are unimportant to this study, merely being an aid in the identification of a ship. This appears to be debatable; most archaeologists would agree that a ship is a cultural component all of whose details should be faithfully recorded. Artifacts by their dates (coins) and markings (cannon) gave an approximate date to the ship and indicated its nationality which enabled the author to track it down through English Admirality records as being, very likely, the H.M.S. *Loo* lost on the Florida Reef in 1743. Here artifacts served a purpose, but otherwise they are virtually ignored. One wonders what was the proportion of Oriental porcelain to English pottery and what other exotic ceramics were used by the crew of an English warship.

On the other hand the historic results are perhaps not so spectacular as they may appear. Florida scholars have long been aware that Looe Reef got its name from a wreck. As early as 1775, Bernard Romans, giving sailing direction for the Florida Keys, mentions "3 leagues off is another [dry sand bar] on which his majesty's ship, the Loo, was lost, from which accident it has acquired the name Key-Loo" (Romans 1775: xxxvii). Elsewhere he quotes Robert Bishop who spells the name Looe, the version appearing on modern charts (Romans 1775: xlvii). And again in the same work, Romans (1775: 297) criticizes De Brahm for mistakenly changing Key Loo to Key Loup on his map.

A second example is the popular report on the famous Roman wine ship of Marcus Sestius, found off Marseille. To remove the mud and other debris an airlift or suction pump was used which sucked up silt and small artifacts or fragments to the surface where the expedition's archaeologists watched for artifacts in the screen. Cousteau (1954: 13) blithely remarks that "when an amphora neck jammed in the [underwater intake] pipe mouth, another diver with a hammer pulverized the obstacle." Is urgency an adequate excuse for this? When not crushing up artifacts the gay divers, judging from the above account, spent much of their time playing jokes on the surface archaeologists, such as shoeblacking pots.

No criticism of the archaeologists for not being underwater is implied; many people simply cannot dive. In this instance with the later establishment of underwater television the expedition's archaeologists were able to participate more closely with the divers. Then, too, they made a brilliant contribution, by the identification of the owner of the wreck. Also the divers deserve great credit for working under unusual conditions. Nevertheless, when we

look at the over-all picture we are faced with but one question: this expedition certainly salvaged a quantity of artifacts, but was this archaeology?

Let me climax these two accounts by defining "underwater archaeology" as the recovery and interpretation of human remains and cultural materials of the past from underwater by *archaeologists*. This last word should be stressed. The problems surrounding cultural materials underwater are just as significant as those on land and they should be handled by trained archaeologists, not by sport or professional divers.

Professional archaeologists should emphasize the distinction between underwater *archaeology* and underwater *salvage*. Both techniques bring artifacts from under the sea. One is a science and the other is collecting. On land any enthusiastic person who digs in an Indian mound modestly becomes an "amateur archaeologist." In the broader dimensions of the underwater world such a person appears to automatically become an "underwater archaeologist."

Perhaps the major problem in underwater archaeology at this time is the lack of trained personnel. Despite adequate financing we have cases where an archaeologist must utilize a diver who works below and reports back to the archaeologist on the shore. In many instances there are no alternatives, but we must face the fact that this is comparable to the archaeology of some years ago when the archaeologist "Bwana" sat in the shade and examined the antiquities his foreman brought in from the dig. Even the closed circuit underwater television camera used in France (Cousteau 1954), by which the archaeologist on shore or ship can watch his divers below, is not an adequate substitute. It is far easier to teach diving to an archaeologist than archaeology to a diver!

The sports diver with an archaeological inclination cannot be ignored. He is increasing rapidly in numbers and like pot-hunters on the land some create problems. Doukan (1957, 247–50) candidly discusses this problem in France. It exists elsewhere as well. Careful cultivation of sports divers may develop some ideal allies.

Situations and Artifacts

Any body of water in the world is a potential site for underwater archaeological work. In practice, though, desirable areas are somewhat limited. Lakes, bays, and streams with soft mud or silt bottoms in which artifacts sink deep out of sight are extremely difficult to work. Tropical waters in which coral effectively blankets every object also present considerable difficulties. Then, too, stained or silty waters create problems not easily surmountable even with strong artificial light. Ideal areas for research are lakes, streams, and springs with clear water and hard sand or rock bottoms. These are not common as a rule but are typically found in limestone areas such as Florida. It should also be kept in mind that stream and sea bottoms are not stable. Periodic flooding of rivers changing the rate of flow may cover or expose material on the bottom. Shifting sands along the seacoast are also changeable. It may be very profitable to check potential sites periodically to see if changing conditions have exposed material and to recheck known sites for new material.

Artifacts from underwater have in themselves certain intrinsic advantages, but because of their situation they may become more difficult to handle than those on land. As already mentioned, in tropical waters coral is a constant nuisance as it grows on any alien material and effectively covers everything. Pure coral is easily removed by various dilute acids, but the presence of other materials may complicate the situation. By experience, one learns that nothing is more resistant to mechanical action or solvents than a mass of iron-impregnated coral surrounding rusted iron bolts, hanks of tarred rope, and thin fragments of porcelain. Freshwater produces no coral problem but algae found there do cover artifacts with a coating which is highly resistant to anything but abrasive action, which is usually impracticable to apply. Iron and manganese salts also stain materials, completely changing and darkening bone, flint, and pottery.

Nevertheless, specimens from underwater often have qualities not commonly found in those on land. For example, broken pottery is usually found in fragments the size of its original breaking, not reduced to the usual small sherds by many trampling feet. As illustrative of this situation, it is interesting to point out that for some of our Florida pottery types the only whole or restorable vessels, or in other cases the best vessels, are those from underwater. The same is true for much of our Colonial material. It is hardly necessary to add that

under certain conditions perishable materials are far better preserved under water than on the land. At Fig Springs, Florida, we found corn cobs, peach pits, gourd and woven cane matting fragments, as well as worked wood and hide. None of these would normally have been preserved in a land site. Charles Brookfield (1941) even found a fragmentary page of a printed bible preserved in coral on the wreck of the *Winchester* sunk off Florida in 1695.

Archaeologists working on the land must wonder about the application of certain fundamental archaeological concepts, such as stratigraphy and context, in dealing with underwater materials. In general it can be said that stratigraphy will not be a major factor in underwater archaeology. I can conceive of situations where it could exist and with skillful work could be revealed. However, such situations are probably rare and opportunities to work them out even scarcer. This is not to mean though that chronology is lacking. The discovery of successive occupied underwater strand lines is very possible at some future date. Context is an important aspect of underwater archaeology. Much of our underwater material is virtually contextless, such as random objects from the bed of a stream. On the other hand, some materials occur in as tight a context as any on land. The context involved can be best illustrated by examining the major types of sites.

UNDERWATER ARCHAEOLOGICAL SITES

Underwater sites fall into somewhat different classes than those on land. They exhibit a great variety of situations, but most can be placed in one of four groups with some random forms. The major groups are: (1) refuse sites; (2) submerged sites of former human occupation; (3) shrines or places of offerings and interments; and (4) shipwrecks.

Refuse Sites. Perhaps the most common of all underwater archaeological sites are those where cultural materials have been deliberately cast into the water. Anyone who has ever lived on a shore knows how typical it is for man to throw his trash into any adjacent stream, lake, or sea.

A variety of refuse sites can be recognized. First are those similar to Oven Hill (Di 15), an 18th century Seminole site on the Suwannee River, Florida. Here people lived on the river bank and simply tossed their refuse into the stream. Such things as cracked whole vessels, and halves, thirds and quarters, of vessels would be a nuisance underfoot but easily disposed of in the stream. Sites such as these are probably far more common than generally realized.

The Cenote of Xlakah at Dzibilchaltún, Yucatán (Andrews 1959; Marden 1959) seems to have served primarily as a water hole, most of the numerous vessels or fragments found in its depths being thin-walled water jars probably broken on the rock rim while being carelessly filled. This is a classical situation of a "pitcher being broken at the well."

Another type of site is exemplified by Fig Springs (Co 1), on the Ichtucknee River, Florida. Here from a small spring we have recovered several thousand Indian and Spanish artifacts (mostly broken) along with refuse such as peach pits and corn cobs. Yet we have been unable to find the original occupation site in the heavily forested vicinity. This would appear to have been a deliberate refuse dump some distance from the mission village site.

In sites such as these, precise context is a problem as there are few clues to distinguish material of one period from that of another; yet in each of the three sites mentioned materials from more than one period were present. We must therefore isolate material from different periods by various techniques. Typology may be useful if the cultural materials are well known, but this will depend on local situations. Weathering may also be helpful. For example, at Oven Hill there was a scant Deptford and Weeden Island occupation (pre-Seminole) of the site. Some sherds of those periods were also found in the river. However, they were so badly worn and eroded in contrast to the Seminole material that they could easily be separated on this basis as well as by typology.

Submerged sites of former occupation. This type of site has perhaps the greatest romantic attraction for the general public. Visions of a sunken Atlantis with colorful fish swimming through ghostly marble pillared halls catch the imagination of many. While we can probably expect to find few classical cities under the oceans, nevertheless we must recognize that a significant number of sites of human occupation are now below sea level.

Man has always found it pleasant and practical to live on the seashore and during the million or so years that he has been on this earth

the edge of the sea has fluctuated greatly. With few exceptions the shore lines during the times of glacial advances are now under many feet of water. For some periods of man's cultural history there may well be far more data under the sea than on the land. A systematic plotting of the sea bottom could lead to the mapping of submerged strand lines. The underwater exploration of these probably would reveal sites. Many of these sites will prove to have been destroyed by wave action when the inundation was gradual. Others covered during a more rapid rise may be less disturbed. The context of material in these will depend upon their degree of destruction.

This may sound like casual speculation but some data now available indicate a strong possibility that there are more sites of this type than has generally been believed possible. Certainly the situation in reverse is not uncommon, that is, the presence of sites on old strand lines now above sea and lake level. As illustrative of this site form we can note that for a number of years reports have been increasingly coming in of the presence of Indian artifacts on submerged sand bars some distance off the coast of Florida in the Gulf of Mexico. It is interesting that the only material reported consists of flint artifacts and flint scrap suggestive of a pre-ceramic culture of some antiquity. Some data on such a site were recently presented by Wilfred Neill in an unpublished paper given at the annual meeting of the Florida Anthropological Society.

Shrines or places of offerings and interments. In this category we can bring together sites of different types whose common denominator is that they are bodies of water into which offerings have been cast by man. It is in these sites that underwater archaeology makes its most original contribution. Perhaps the most spectacular of all underwater shrines is the Sacred Cenote at Chichén Itzá, Yucatán. Here with crude dredging and diving suit techniques a wealth of Mayan ritual and ornamental objects was recovered by Edward H. Thompson (Lothrop 1952; Tozzer 1957). Undoubtedly refined techniques in the future will recover much more, although the technical problems of working in this cenote have yet to be solved.

Within the same category we can include Little Salt Spring (So 18) in Sarasota County, Florida, recently discovered by Louanna Pettay. On the sloping sides of this cenote spring, at depths of 25 to 35 feet, is a large deposit of human bones. Preliminary reconnaissance has yielded the remains of more than 50 individuals with every indication that many more are present. The bones are lightly mineralized and lie at random under a thin layer of shelly detritus with no evidence of articulation. At this stage of research we believe that cleaned unarticulated bones were thrown into the spring by prehistoric Indians as a variant on the normal practice in this area of secondary burial in mounds. However, few artifacts have been found. Until more and distinctive artifacts are recovered the dating of the bones seems impossible. It is hoped that a detailed study of this site can be made in the near future.

In sites such as these, unless there are typological distinctions in the material, it is difficult to determine whether we are dealing with a deposit made at one time or over a period of years. On the other hand, each of the numerous groups of incense burners and other objects found by Borhegyi (1958, 1959) in Lake Amatitlan, Guatemala, seems to be a discrete unit. These groups have in some cases been equated with material from sites on the nearby shore and they date over a long period of time showing that the custom of water offerings among the Highland Maya is of long and consistent duration.

Shipwrecks. Shipwrecks can be divided into two major types, vessels that founder and settle relatively quickly to the bottom and those that break up on striking rocks, reefs, or the shore. Thirdly, but less important, are vessels that have collided and sunk.

Those that foundered have proven to be ideal for underwater archaeology as they usually settle quickly to the bottom with their contents and context completely intact. The Greek and Roman merchant vessels found in the Mediterranean Sea with cargoes of amphoras still lying in their original neat rows are classic examples of this situation (Cousteau 1954; Doukan 1957: 217–50).

Vessels that strike a reef may fill with water and sink onto the bottom with cargos intact but mixed and broken from the impact. Some may drive over the reef and sink while others may break up with sections drifting apart and spilling cargo and gear along the bottom for hundreds of yards. The internal context of vessels that strike on reefs or shore is usually badly jumbled as the shock of striking throws every-

thing heavy forward, such as guns, packaged cargo, and ballast, smashing and pushing along in front of them any lighter materials.

Of all types of sites shipwrecks probably offer the tightest situation for the underwater archaeologist in terms of context. Generally speaking, a wreck is a delimited cultural unit uncontaminated by material from another time. Nevertheless, wrecks in harbors or other places with much traffic may well have later artifacts fall upon them. The superimposition of wrecks of different periods at critical spots has been noted in the Caribbean by the Links (Link 1959: 67–8), and Cousteau (1954: 16) noted later material on the top of the Roman wine ship of Marcus Sestius.

Miscellaneous situations. Cultural materials also occur in a variety of underwater deposits, some of which are still unexplainable. All bodies of water seem to have a few lost or randomly discarded artifacts although in some cases it is difficult to interpret a specific situation. At Oven Hill, for example, along with Seminole artifacts of the late 18th century and modern bottles and beer cans we found a Civil War fused hollow shot. Yet we know of no naval engagement in this area!

In Lake Amatitlan, Borhegyi (1959: 108) found groups of upright incense burners imbedded in lava. Apparently they had been placed on flowing lava on the shore and then gently moved with the lava into the lake.

One of our puzzling problems in Florida is the presence of numerous bi-pointed bone projectile points deep in swift flowing spring caves. At Wakulla Springs more than 600 have been reported occurring up to depths of 200 feet (Olson 1958: 402). We have found over a dozen at a depth of 55 to 60 feet back in the spring cave at Blue Hole (Co 17), Ichtucknee River, Florida, which indicates that Wakulla Springs is not an isolated example.

Some of the most difficult underwater sites to interpret are the many finds of Indian stone mortars, pestles, and net weights off the California coast in the vicinity of San Diego. These occur in groups of up to over 300 specimens (mortars) at depths of 20 to 100 feet and up to one-half mile off shore. Tuthill and Allanson (1954) consider a number of possibilities such as deliberate sacrifice and the submergence of the sites by a rise in sea level but finally explain them by coastal erosion. That is, the coast was eroded away leaving the artifacts in their more or less original place of deposition. This is somewhat difficult to believe, but in the absence of experience on the California coast, it must be left to the archaeologists of that area to arrive at a final conclusion.

CONCLUSIONS

A number of points can be emphasized. First, and perhaps most important is the conviction that underwater salvage and underwater archaeology are not the same thing. Probably most of what has been called "underwater archaeology" to date is really only salvage. Archaeologists must develop diving skills and take over the responsibility for underwater remains. Underwater archaeology will not supplant conventional archaeology, but will tend to complement it. In general this will be by the recovery of artifacts in better condition than those usually found on land. However, in certain types of sites such as shrines, underwater archaeology may make its own unique contribution. The usual controls in the handling of material on land have their counterparts in underwater archaeology. Careful attention of these will often yield good context for the remains found. It is also clear that various special techniques will have to be devised to deal with the special problems found underwater.

Acknowledgments. Research at Fig Springs, Oven Hill, and other sites in the Suwannee River drainage was carried out with the help of several grants from the Wenner-Gren Foundation for Anthropological Research (New York), and the Graduate School and College of Arts and Sciences of the University of Florida.

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MAYA SETTLEMENT PATTERN IN NORTHEASTERN PETÉN, GUATEMALA

WILLIAM R. BULLARD, JR.

ABSTRACT

A reconnaissance of Maya ruins in northeastern Petén, Guatemala, reveals a heavy but dispersed type of settlement during the Classic period, with the dispersion conditioned principally by availability of water supply and by suitable terrain for building. The types of archaeological remains described are House Ruins, Minor Ceremonial Centers, and Major Ceremonial Centers. Settlement is seen as having been organized into (1) "clusters" of from five to 12 houses, (2) aggregates of clusters forming township-like zones of from 50 to 100 houses, each of which had a Minor Ceremonial Center as its functional nucleus, and (3) aggregates of zones forming large province-like "districts," each of which had a Major Ceremonial Center as its functional nucleus. These units are described and discussed. Some consideration is also given to problems of water supply, population, and agriculture. Reference is made to the ruin of Topoxté, a town or small city of the Postclassic period located on a group of islands in Lake Yaxhá.

THIS PAPER presents the results of a preliminary survey of ancient Maya settlements in the northeastern part of the Department of Petén, Guatemala, carried out during 1958. The specific area and itinerary of the survey are shown in Figure 1. This region was an important center of Classic Lowland Maya civilization. Located within it are the famous ruins of Uaxactún, Tikal, Naranjo, Nakum, Holmul, Yaxhá and others. Briefly stated, Maya occupation began probably in the first millennium B.C. Based on agricultural exploitation of the tropical forest environment, the culture flourished and the population grew until the close of the Late Classic period, about A.D. 650 or 900, depending on which Maya-Christian calendar correlation is used. The region was then entirely or almost entirely abandoned except for a limited Postclassic occupation principally around Lake Petén-Itzá.

Through the researches conducted mainly by the Carnegie Institution of Washington and the Peabody Museum of Harvard University, we now have considerable knowledge concerning chronology and the ceremonial aspects of Maya culture in Petén, but there are few data or little general agreement concerning the nature of domestic settlement and the relationship of house ruins to ceremonial ruins. Previous work on the subject is limited to Ricketson's survey of house mounds in a measured area in the environs of Uaxactún (Ricketson and Ricketson 1937: 15–24, Fig. 2) and the Belize River settlement pattern project directed by Willey in western British Honduras, across the border from Petén (Willey, Bullard, and Glass 1955, and report in preparation; Willey 1956b). As Willey (1956a: 110) has pointed out, the Uaxactún survey has been used as evidence for both a dispersed rural type of settlement and a loosely compacted urbanism. The Belize River settlements, because of the particular river conditions of that region, have been regarded as probably atypical of the Maya lowlands as a whole (Brainerd 1956: 162).

The specific aims of the present survey were to find out what types of small ruins occurred in northeastern Petén, how and where they were distributed over the countryside, what types of groupings they were in, and their relationships to the large ceremonial ruins. In this connection, it was necessary to work not only near ceremonial ruins but also well away from them. The survey was designed to be extensive rather than intensive in scope. That is, the idea was to get around and see as much as possible of settlements throughout the region rather than to make a detailed study of a small area. This approach has admitted limitations. It cannot provide the accurate quantitative data that result from careful mapping and test excavating in a small area. But it can give an over-all characterization and perspective of the settlement situation which is lacking for this region, and which I believe should precede and complement more intensive survey.

The greater part of the work was done at two locations, Lake Yaxhá and Dos Aguadas. In addition, a large amount of territory was covered by trail with frequent stops of several days' duration along the way in order to examine special types of terrain. Archaeological survey in a practically uninhabited, inadequately mapped tropical forest faces difficulties not encountered in more open country. Chief among these is the forest itself. Even under the best of conditions it is seldom possible to see a small ruin more than 15 m. away, and it is often not easy

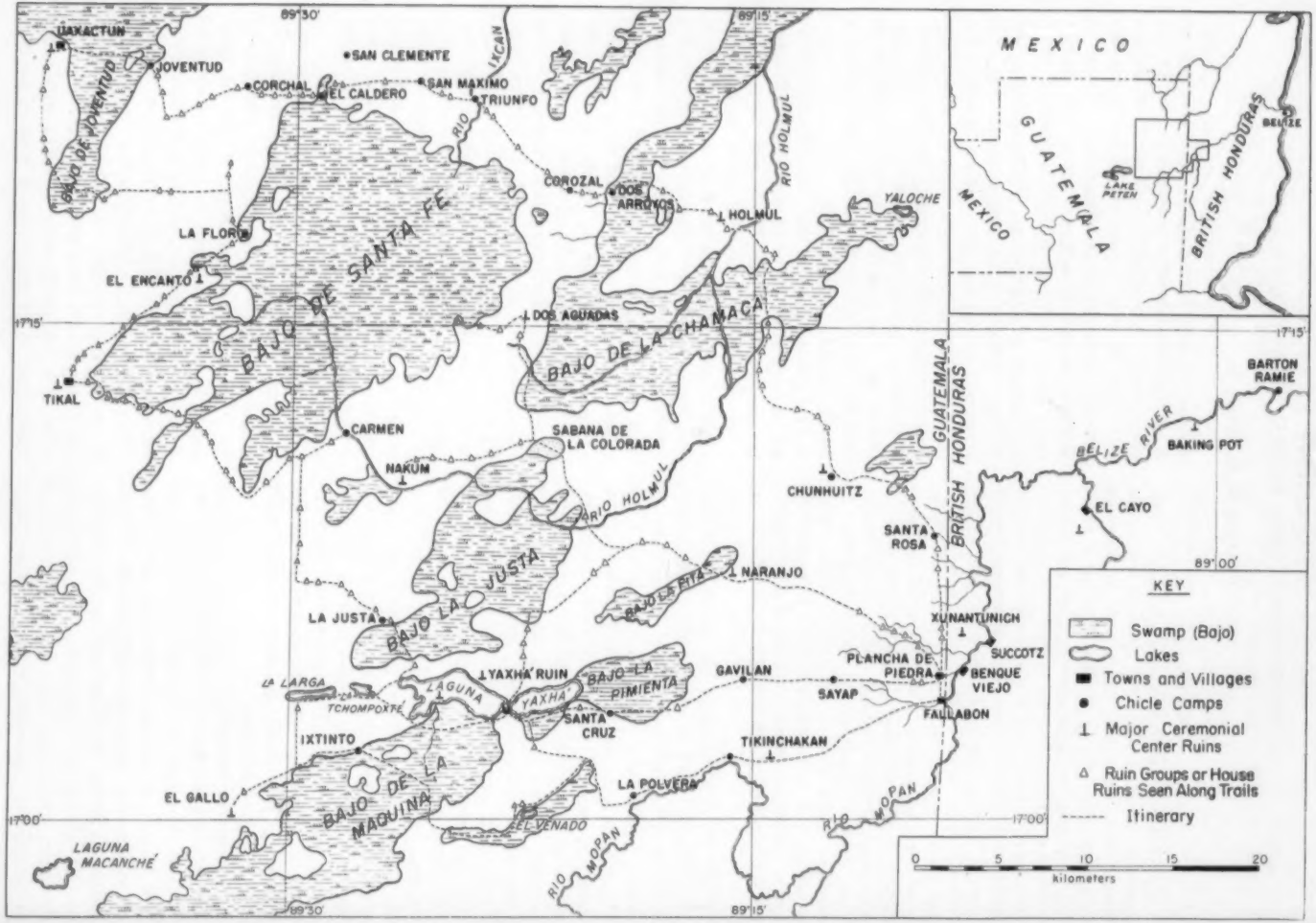

FIG. 1. Map of northeastern Petén and adjacent British Honduras showing the route of the survey.

to get a good idea of the lay of the land in which one is working. The principal method employed was simply to explore systematically, often with compass traverses, the selected areas until the general settlement situation therein could be adequately determined. At Yaxhá and Dos Aguadas, the exploration work was supplemented by the detailed recording of a number of surveyed plots, mostly arbitrarily selected, and by some test digging.

ENVIRONMENT

Northeast Petén is a heavily forested limestone country. Physiographically, it is composed of uplands separated by great wooded swamps known as *bajos*. The uplands are principally rolling country characterized by level-topped hills and ridges dissected by often steep-sided little valleys and interspersed with small areas of swampland. There are some ranges of steep knobby hills but none reach the proportion of mountains. The uplands are blanketed with luxuriant high forest in which the outstanding trees include mahogany, Spanish cedar, sapodilla, *ramón* (breadnut), and several varieties of palm. The *bajos* are altogether different. These are flat basins often several miles across covered with dense scrubby growth. Generally speaking, the northern and western edges of the bajos lie at the foot of long steep escarpments rising sometimes 100 or more meters in height, while the southern and eastern edges are less well defined with considerable interfingering of swampland and upland. After periods of heavy rain, water stands on the *bajo* surfaces; in the dry season they dry completely and the clay soils become almost cement-like in hardness. In the area of the survey, an estimated 50% is *bajo* or smaller areas of swampland, and thus is suitable for neither habitation nor agriculture.

Northeast of Lake Petén there are only a few permanent lakes, the largest of which is Lake Yaxhá. With the exception of the Río Mopan, a tributary of the Belize River, the few rivers and streams in the area of the survey, including the long Río Holmul, are intermittent. The principal water sources are the *aguadas*, small depressions which retain rain water. They are scattered here and there through the uplands and around the edges of the *bajos*. As will be described, many of the *aguadas* were made by the ancient Maya. Rainfall varies from year to year, averaging probably about 60 inches. During the rainy season from about May to January water is usually no problem, but during the other months of the year the *bajos* and many of the *aguadas* dry completely and the rivers and larger streams either dry or shrink to a series of well-separated pools. Dry season desiccation is a limiting factor for modern settlement in Petén and forms a problem in the study of the ancient occupation.

Today, most of the population of northern Petén lives around Lake Petén, the rest of the area being mainly uninhabited. In the area of the survey, the only permanent settlements are at Fallabón–Plancha de Piedra, Uaxactún, and, owing to the recently inaugurated archaeological work, at Tikal. A village which existed for many years at Lake Yaxhá was abandoned in 1945. During the rainy season, chicle collectors scatter in temporary camps through the forest in order to tap the sapodilla trees. During the dry season, there is some mahogany cutting by British Honduras operators, and a program of oil exploration has started recently. There are as yet no permanent automobile roads. The few temporary "truck passes" made for the mahogany and oil work become impassable in the rains and have to be cut open anew before every season of use. Mule transport remains the most reliable and practical means for archaeological survey.

ARCHAEOLOGICAL REMAINS

Structural remains encountered in northeastern Petén may be classified in broad terms as (1) House Ruins, (2) Minor Ceremonial Centers, and (3) Major Ceremonial Centers. Additional remains include chultuns, quarries, artificial *aguadas*, a well, and a flint-working site. Except in certain of the largest ceremonial ruins, buildings are in an advanced state of ruin and determination of function must be derived from surface appearance alone. The majority fall quite easily into the above three classes, but it is only to be expected that occasional individual examples will be difficult or impossible to place.

House Ruins

The great abundance of the small ruins which, as a group, are classed as house ruins leaves no doubt that the majority were, in fact, the residences of the common people. Nevertheless occasional individual structures, particularly those which are slightly larger than normal or otherwise architecturally distinct, may have

FIG. 1. Map of northeastern Petén and adjacent British Honduras showing the route of the survey.

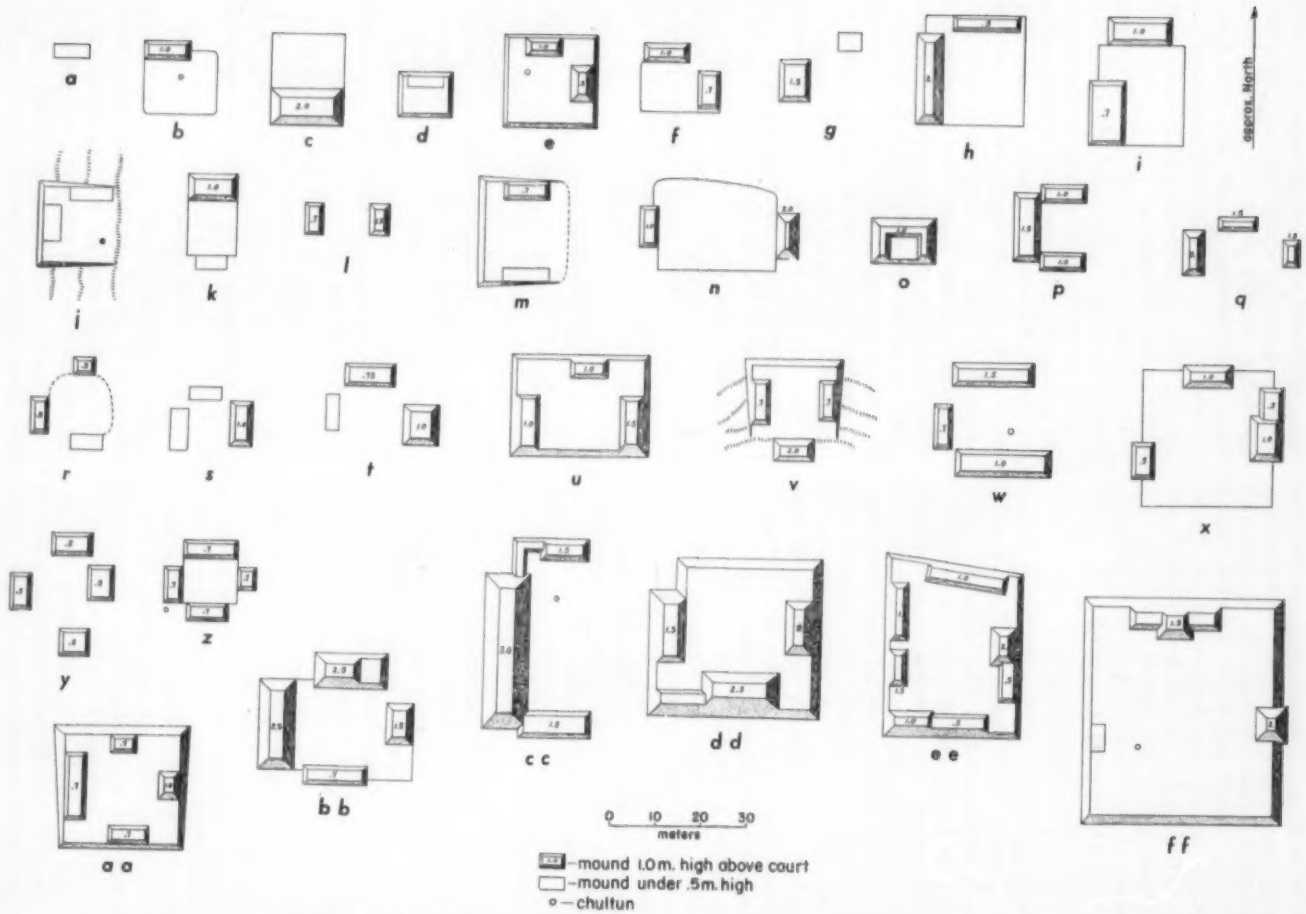

FIG. 2. Representative house plans from Yaxhá and Dos Aguadas. *a–bb*, ordinary house ruins; *cc–ff*, large house ruins prominent in cluster type of groupings.

actually served specialized purposes. Even excavation may be unable to determine satisfactorily whether such a ruin was a large residence or a small public or ceremonial building.

The ordinary ancient Maya house was of perishable construction, probably basically similar to the pole and mud-walled, thatch-roofed houses used by the present-day Maya. The surviving remains are the terraces and substructure platforms upon which the houses themselves stood. The platforms appear as rectangular mounds usually with one axis longer than the other. These occur singly or in units of two, three, or four (rarely more) around an often raised court. In this paper, each of the units so formed is considered as an individual "house," but it should be understood that units composed of two or more platforms may actually have included more than a single dwelling structure. In my samples, units composed of one, two, and three platforms are numerically nearly equal; units with four or more platforms are definitely less numerous.

Figure 2 illustrates a representative series of house ruins from Yaxhá and Dos Aguadas. Some mounds are of almost imperceptible height; the largest may be as high as 3 m. above their courts. Usually one mound is higher than the others in the same unit. The terraces upon which the separate platforms stand vary in height according to the terrain but are usually not over about one meter on level ground. Houses built on slight slopes may have one side of the court cut into the slope while the opposite side is raised (Fig. 2 *j*, *v*). Even where terrace edges are not visible above ground the courts are usually level and were probably paved. Occasional examples were seen of terraces which had no visible remains of smaller platforms on their surfaces. Chultuns sometimes occur in house ruin courts. The structures are usually more or less oriented to the cardinal directions.

Subordinate architectural features such as masonry wall bases and benches are ordinarily not visible on the surfaces of Petén house ruins, but excavations at Uaxactún (Wauchope 1934) and at Barton Ramie in the Belize Valley have shown that they can be expected to occur. Test pits and other exposures of mound interiors show fill composed of stones, small stones and earth, or sometimes packed earth. Retaining walls were of small shaped stones sometimes with unshaped boulders of hard limestone used for the basal course. Platform and court floors were of lime plaster underlaid with pebbles. One example was seen of a house platform in part cut from a bedrock ledge.

In nearly all areas explored, there were some ruins which were so low and indefinite that no plan could be made out. Their identification as structures not infrequently depended on whether we could turn up potsherds in the soil about. Some house floors were laid directly on the ground surface or were so slightly raised above it that no surface trace at all remains. We located several such ruins by excavating in places where topography, finds of sherds, or other reasons suggested the possibility of a house site. The problem of the relative number of these "invisible" ruins was given attention throughout the survey and it is my belief that comparatively few ancient houses leave no surface trace at all, and that on most types of terrain the probable locations of these can be predicted.

It is possible that a very few house ruins were entirely of masonry with vaulted roofs. Every now and again we came across house ruins, well away from ceremonial centers, which included one or more mounds of such shape and contour as to suggest that they may have been vaulted. The ruins in Figure 2 *n*, *cc* contain examples. A few small partly intact vaulted buildings were seen, although not at either Yaxhá or Dos Aguadas. Some of these were associated with units of normal house-ruin size, but most were with ruins too large to be regarded as ordinary residences.

Occasionally, a comparatively large ruin of the general house-ruin class includes a small pyramidal structure which was probably a shrine. Whether this means that the entire unit in question was non-residential usually cannot be decided by surface evidence. One example which would seem surely to have had an exclusively ceremonial or public purpose is shown in Figure 2 *ff*. It has an exceptionally large raised court in addition to two small pyramidal structures, but it nevertheless is considerably smaller than what are classed as Minor Ceremonial Centers.

Minor Ceremonial Centers

The ruins designated as Minor Ceremonial Centers form a class apart: appreciably larger than the House Ruins, and appreciably smaller than the Major Ceremonial Centers. This does not mean that occasional examples cannot be

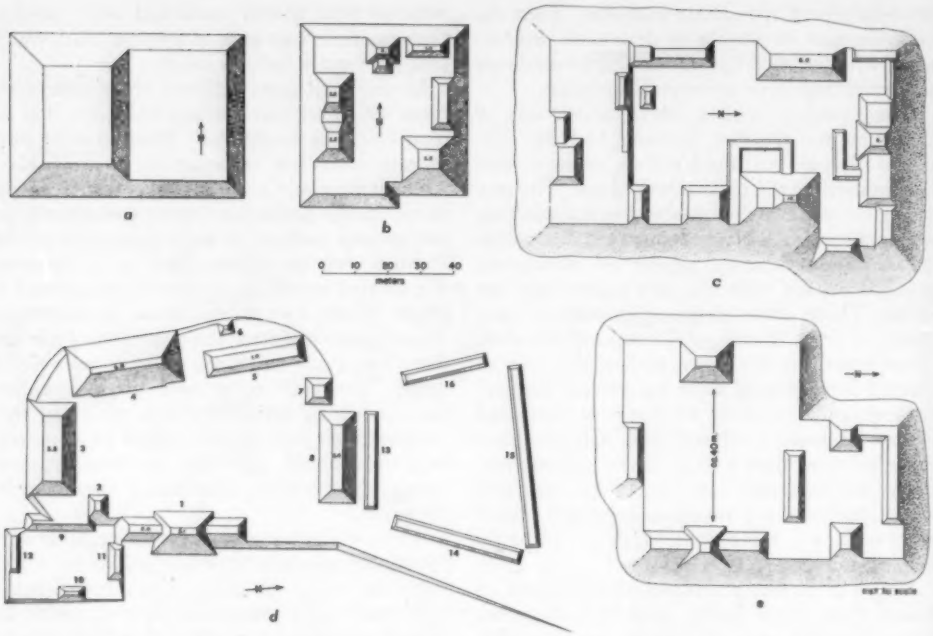

Fig. 3. Representative Minor Ceremonial Centers. *a*, La Flor; *b*, Dos Aguadas F-1; *c*, Dos Aguadas B-1; *d*, Yaxhá Hill; *e*, El Venado.

found which are intermediary in size. Minor Ceremonial Centers may consist of only one large building (Fig. 3 *a*), but ordinarily they include one or more pyramidal structures, which are assumed to have been small temples, arranged in company with lower buildings around one, two, or three adjacent plazas (Fig. 3 *b-e*). Only rarely are the plazas so separated that the center can be considered as having more than one group. Small vaulted buildings of "palace" type may be present but they do not form the often extensive compounds which are usually associated with Major Centers. None of the many Minor Centers explored during the survey contained stelae, altars, or ball courts.

The only example seen of a Minor Center which was approached by a graded causeway (*sacbe*) is located near San Maximo Aguada. Careful search of the vicinity failed to reveal other ceremonial buildings. The *sacbe* terminated in an area of scattered house ruins.

The broad flat surfaces of the pyramid in Figure 3 *a* were featureless. In Figure 3 *d*, mounds 9–12 suggest an attached residence. The long low mounds (13–16) are an unusual feature. A somewhat similar arrangement occurs in another Minor Center near Yaxhá. Perhaps they were market enclosures.

Major Ceremonial Centers

The Major Ceremonial Centers are the great ruins for which northeastern Petén is justly famous. Located within or near the area of the survey are Tikal, Uaxactún, Naranjo, Nakum, Yaxhá, Holmul, San Clemente (El Gallo), Chunhuitz, El Encanto, Benque Viejo, and others. Plans of most of the above have been published (see especially Morley 1937–8, Vol. 5). Most were visited during this survey.

The Major Ceremonial Centers vary greatly in size but all are substantially larger than the Minor Centers and contain larger, more elaborate buildings. Outstanding types of buildings are temples, often with vaulted roofs, on high pyramidal, terraced substructures, and lower but often extensive multi-roomed vaulted buildings called "palaces." Stelae and altars are usual features and, particularly in the larger ruins, many are sculptured with human figures and hieroglyphs. Most Major Centers in this region also contain at least one ball court. Many Major Centers include more than a single ar-

chitecturally independent group and where the groups are not too far apart they are not infrequently joined by graded roadways. Artificial reservoirs have been found in the two most carefully explored large centers, Tikal and Uaxactún.

Two previously uninvestigated Major Centers examined during the survey were at Dos Aguadas and Tikinchakán. Dos Aguadas (Fig. 4) was seen in passing by Morley in 1916 (Morley 1937–8, Vol. 1: 88) but was not thoroughly explored at that time. There are no intact standing buildings and the eight stelae and three altars which we located are without sculpture. Structure XXIII has a circular outline but its ruined condition makes it uncertain that this was the original shape.

Tikinchakán ruin consists of two groups overlooking the Mopan River Valley. One group is composed of a pyramid surrounded by

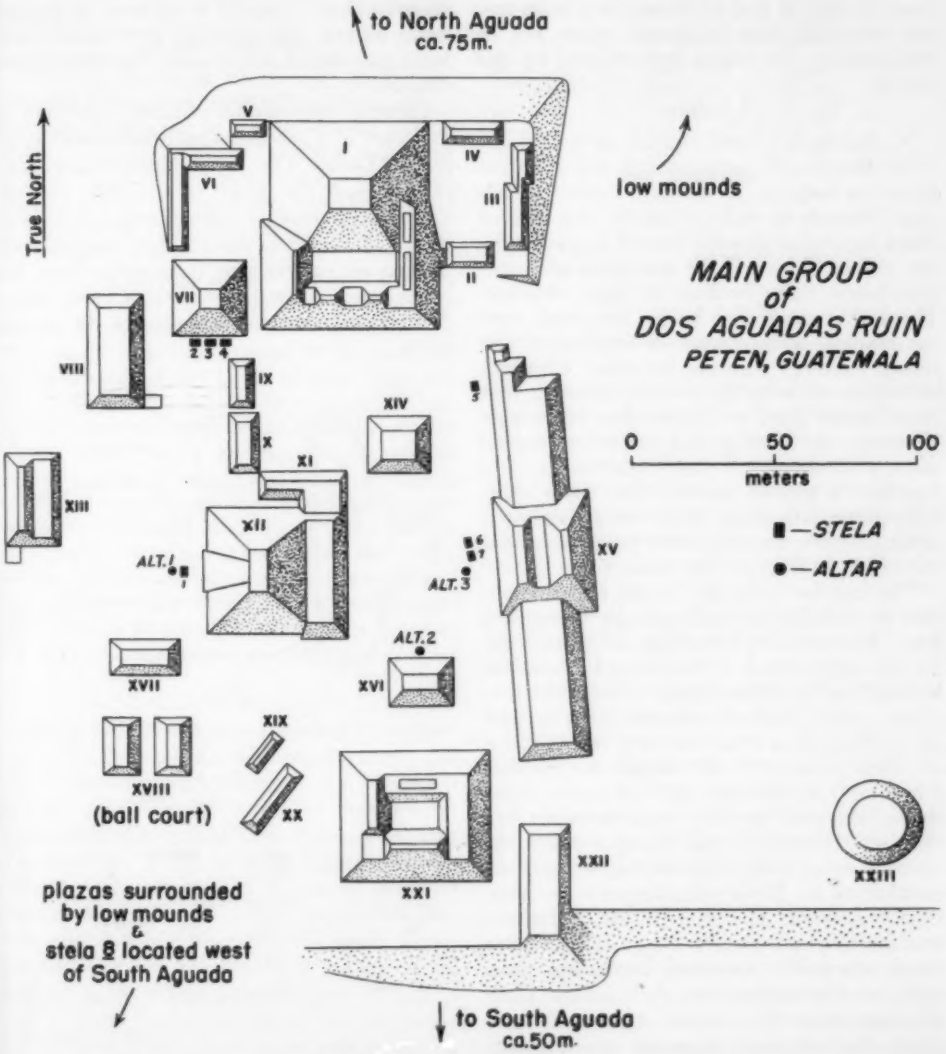

FIG. 4. Plan of Dos Aguadas ruin.

ruined vaulted palaces. The other is a broad plaza on which are situated two palace-type buildings, one of which includes a magnificent standing vaulted room 32 m. long. There appears to be one fallen stela, probably plain, associated with this second group.

Dos Aguadas and Tikinchakán ruins are both small compared to many of the other Major Centers recorded in northeastern Petén. It is probable that other Major Centers comparable to these two will be discovered, but I think it unlikely that additional very large centers with numerous sculptured stelae will be discovered in the region encompassed by this survey.

Chultuns

A *chultún* is a bottle-shaped pit with a circular manhole-like entrance dug into the solid limestone bedrock. In northern Petén they are very frequent in areas settled by the ancient Maya in places where the bedrock is close under the surface. Most can be associated with specific house ruins because of their nearness. Those that could not be so associated were nevertheless within areas of settlement and ruined buildings were not far away. Chultuns sometimes occur in house ruin courts, a trait noted more often at Yaxhá than elsewhere. Chultuns also occur in and around ceremonial centers. Often two or more chultuns are close together. It may be assumed that many chultuns, particularly those which retain their stone covers in place, are completely concealed under the soil and detritus of the forest floor.

The circular openings in the bedrock surface are usually just wide enough to admit a man. The necks are cylindrical or have a ledge for the stone cover. If the bedrock is covered by much soil or if the chultun is in a house ruin court, a short shaft of masonry connects with the surface. The stone covers, if not in place, are often found in the fill beneath the opening. Chultuns may contain only a single dome-shaped chamber but more often there are two or more. Commonly, the chamber below the opening serves as an entry to deeper chambers opening off it. These side chambers are often roughly squared. The principal use of chultuns was probably as storage places, although some are known to have contained burials and there could have been other uses. A. L. Smith (1950: 85) suggests that the chultuns served initially to obtain the soft marl or *sascab* which occurs beneath the hard surface limestone. Although the marl obtained during their construction was probably used, I think it doubtful that their main purpose was as marl pits since the interiors are often carefully finished and they are found close to as well as far from larger quarries which were clearly made for obtaining *sascab*. That they were not used as water cisterns as were the larger chultuns of the Puuc region of Yucatán (E. H. Thompson 1897) is evident from the lack of the necessary impermeable lining and provision for drainage into the chultuns, their presence close to as well as far from permanent water sources, and their frequent multi-chambered plan which would make drawing of water difficult.

Figure 5 represents a chultun excavated in the court of a probable small ceremonial structure at Yaxhá. The chultun itself is quite typical although the length of the shaft through the high terrace is exceptional. Associated pottery showed that this chultun was originally constructed in Preclassic (Chicanel) times, but it remained in use through the Classic period during successive enlargements of the terrace above.

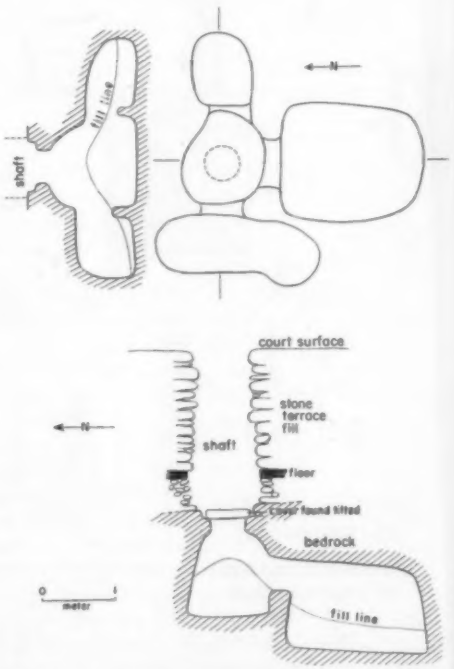

Fig. 5. Plan and cross sections of chultun E-1 at Yaxhá.

Quarries

Two types of quarries were found. The first type consists of exposed limestone strata with straight vertical faces from which building stones have been cut. The second type are cave-like excavations made for extracting the soft white *sascab*, used in making mortar, which occurs beneath the hard limestone crust. Some quarries have piles of rock detritus on one side. Quarries are sometimes found in house-ruin areas but are more frequent near ceremonial centers.

Artificial Aguadas

Many of the more permanent *aguadas* in northeast Petén show unmistakable signs of having been constructed or enlarged by the ancient Maya. They are generally circular basins about 50 to 100 m. in diameter dug into the clay soil of swampy places, the excavated earth being piled in a dam-like embankment around the rim. They impound rainwater and in some instances run-off from adacent slopes. Artificial *aguadas* occur in patches of swamp or low ground in upland areas and also in narrow coves at the edges of the large *bajos*. Some of the latter are in the centers of the coves so that when the *bajo* surface is flooded in the rainy season the *aguada* rim protrudes as a doughnut-shaped island. Some of the upland *aguadas* are on slightly sloping ground so that when the *aguada* is filled the water retained by the *aguada* embankment is higher than the ground surface on one side. No suggestions of stone work were evident around any of the artificial *aguadas* examined during the survey, but I am informed that Aguada Pedregal, north of Dos Arroyos, has stones visible around the rim.

Well

A well located one league north of Dos Aguadas on the trail to La Golondrina is the only feature of its kind encountered on the survey. It is a circular shaft 0.85 to 1.0 m. in diameter penetrating the ground to a depth of at least 5 m. (Fig. 6). The stone lining is approximately 60 cm. thick composed of unshaped stones of varying size laid up without mortar. No bedrock can be seen in the side of the shaft. When the well was first investigated at the end of the dry season of 1955 it contained over a foot of water. During the dryer May of 1958 we had to remove over 50 cm. of soil before water level was reached. The shaft continued still deeper. The

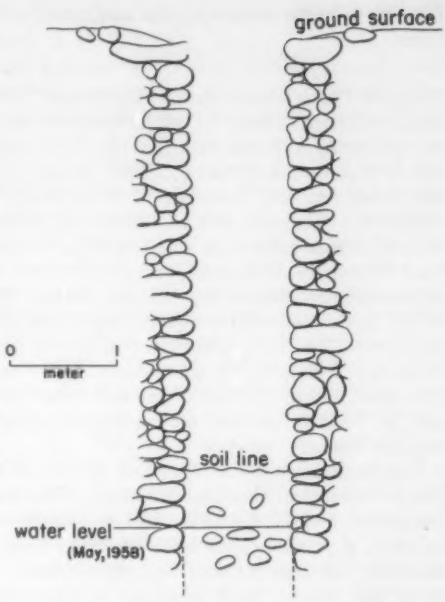

FIG. 6. Cross section of well near Dos Aguadas.

well is located near the foot of a long slope and close to the edge of a stretch of *bajo*, but the *bajo* surface is lower than the water level in the well. Presumably the well penetrates the water table at this point and is supplied by subsurface seepage down the slope. It is noteworthy that at the present time there are no other water sources as reliable as this well within a distance of about 2 k., and for this reason it is occasionally used by chicle collectors and other passers-by. The well is at the edge of a house ruin area and there are indications of some sort of small stone structure adjacent to the well itself, but no plan could be made out.

Prehistoric wells have not hitherto been reported from Petén, but Brainerd (1953: 114) states that the Maya dug wells in the Chenes area of Campeche as early as the Classic period and there is also evidence for prehistoric wells in Yucatán (Tozzer 1957: 3).

Flint-Working Site

Near Santa Rosa on the British Honduras frontier north of Plancha de Piedra, a newly bulldozed road has cut through a flint-working site. The road is strewn with chips and fragments of the percussion-chipped, leaf-shaped core axes which are common in Petén ruins.

The site is on the crest of a ridge and near house ruins.

Age of the Ruins

In the Petén forest, sherds disintegrate after no great length of time on the surface, but more are continually being brought up by animal action and falling trees. Evidence from these and from the few tests which were made is sufficient to indicate that settlement of practically all house-ruin areas had begun by at least Late Formative (Chicanel) times, but was at its maximum during the Classic period (in round figures, A.D. 300 to 900 by the 11.16.0.0.0 correlation, A.D. 50 to 650 by the 12.9.0.0.0 correlation). The only Postclassic settlement encountered within the area of the survey was the ruin of Topoxté situated on a group of islands in Lake Yaxhá.

The settlements to be described are therefore almost entirely of the Classic period. This encompasses several centuries and it cannot be assumed that all houses were occupied simultaneously or continuously for this length of time. But, by and large, available evidence indicates great stability of settlement and re-use of house foundations for long periods. The results of this survey suggest certain chronological problems which can only be solved by an intensive program of test excavation.

General Distribution of Settlement

The frequency and number of house ruins seen along trails in Petén is indeed astonishing and one's first hasty impression is that they are just about everywhere. I kept a timetable record of house ruins seen from horseback while travelling from place to place. These are recorded in Figure 1, the symbols in many instances representing occurrences of several houses too near together to be placed separately on the map. Upon more thorough investigation we find that settlement, although in over-all view dispersed, does not completely blanket the country. Houses tend to concentrate in some places and not in others. Within a particular house ruin area, I would rate density of settlement as relatively low where houses average 150 m. or more apart, as relatively high where they average 50 to 75 m. apart. There is a marked tendency for these house-ruin areas to occur in the vicinities of present-day, and possibly former, water sources in places where the terrain offers suitably large areas of well-drained level ground. Another possible, but less obvious, factor influencing settlement distribution may be accessibility to good farmland.

Houses are on high ground all around Lake Yaxhá, the largest lake east of Lake Petén. Similarly, they seem to occur practically all around the edges of the large *bajos*. They are also found in the vicinities of *aguadas*, both natural and artificial, and near the larger arroyos and stream beds. These concentrations are by no means tight bunchings of ruins. It is simply that houses are more frequent within about a kilometer of such features than farther away. It is my impression that the maximum distance from a water source was probably less than two kilometers. In some localities, the tendency for concentration near water sources is more difficult to observe because of the existence of several scattered sources. I did see house-ruin areas where the potential water supply was not apparent and was unknown to my guides, but in these places we ourselves were unable to stay long enough for an adequate search.

Dense concentrations of houses occur on the flatlands bordering the Mopan River, and houses also follow along the several more or less permanent creeks which flow from west and north of Fallabón across the British Honduras frontier to the Mopan. The Mopan, the only permanent river touching the area of the survey, is a tributary of the Belize River and the settlement situation along it is an extension of what was found further downstream in British Honduras (Willey, Bullard, and Glass 1955). For this reason, the Mopan Valley settlements will be omitted from the remainder of the discussion.

In broad view, settlement fringing lakes and *bajos* and where *aguadas* and water sources are common often seems to be virtually continuous. Actually, the seeming continuum is divided into large and small segments by breaks in the terrain and the availability of suitable building sites. Preferred locations were the comparatively level, well-drained, hill and ridge tops at medium elevations, with houses also found on level spurs on hill slopes, the tops of semi-isolated knolls, level areas at the foot of escarpments, and similar places. Where the ground surface has a slight undulation, the houses typically occupy the high points. Areas of steep slopes and rugged terrain were either not settled or very lightly so. *Bajos* and other areas of swamp or poor drainage were not inhabited, although houses may be found at their very

edges. In sum, the distribution of settlement appears conditioned principally by the occurrence of sufficiently large tracts of well-drained relatively level terrain within a kilometer or so of a water source.

A striking correlation between the presence of Maya ruins and groves of the *ramón* (breadnut) tree was noted throughout the survey, and has previously been commented upon by Lundell (1937: 10). Although ruins were noted in other forest associations, *ramón* is dominant in most areas of ruins and, conversely, ruins will almost always be found where *ramón* is the dominant tree. Lundell believes that the ancient Maya planted *ramón* around their houses for its edible fruit and that its present dominance around ruins is "due to an initial advantage accruing to the species through its presence in large numbers when the places were abandoned."

The Problem of Water Sources

Sparsity of large permanent water sources and desiccation of many *aguadas*, streams, and other surface sources during the dry season is a hindrance to modern settlement of many parts of Petén. It has long been perplexing how the large population of ancient times got through the dry season in areas where even comparatively small modern settlement is precarious. There is no evidence for climatic change of major proportions or of drying up of the region since former times. The same kinds of tropical woods are found in Maya ruins as thrive in the region today. Nor does there seem to have been a significant over-all drop in the water table. For example, house ruins of both the Classic and Postclassic periods are found as close as 3 or 4 m. above the present level of Lake Yaxhá, demonstrating that the level was not substantially higher in the past.

C. Wythe Cooke (1931) has advanced the theory that the great *bajos* were shallow permanent lakes at the time of the ancient occupation. According to Cooke, gradual silting of the lakes, intensified by increased erosion of the uplands which were deforested for agriculture, converted them into the swamps they are today and spurred abandonment of the region. Similar explanations have been suggested to me by observant natives of Petén. The *bajos* themselves, broad flat basins covered with low growth, have much the appearance of bodies of water when viewed from adjacent upland bluffs or from the air.

In apparent support of Cooke's theory is the fact that one type of place where house ruins tend to concentrate is the edge of *bajos*. Moreover, many of the large ceremonial centers overlook *bajos*: Tikal, Uaxactún, Naranjo, and San Clemente, among others. But other factors are involved. Artificial *aguadas* are not infrequently built in the clay soil of coves at the edges of *bajos*. Not only would there seem no need to build water-storage basins next to lakes, but many of the artificial *aguadas* are in places which would have been part of the lakes themselves. In addition to the availability of good places for constructing artificial *aguadas*, *bajo* edges may have had other advantages for settlement. Water remains on the *bajo* surfaces after heavy rains, the bluffs and escarpments bordering *bajos* often contain large arroyos and sometimes springs, and there may have been less obvious attractions.

Nevertheless, the Cooke hypothesis is attractive and I think it plausible that at least portions of such large *bajos* as those of Santa Fé, Joventud, La Chamaca, and La Justa were open water during the initial occupation of the country and even into the Classic period, and that the building of the artificial *aguadas*, necessitated by progressive silting of the lakes, happened in the later part of the occupation. If this were so, we should expect earlier settlement to be scattered all along the *bajo* edges and later settlement to be restricted to those places where there are *aguadas* and other reliable water sources. This idea could be tested by a program of excavations aimed at dating house ruins in selected areas, and it might be possible to procure dates for some of the artificial *aguadas* themselves.

Whatever may be the merits of the Cooke hypothesis, conversion of lakes into *bajos* was probably not the only or the principal reason for the collapse of Classic civilization and the abandonment of Petén, since areas where there could have been no water shortage, such as lakes Petén and Yaxhá and the Belize River valley, seem also to have had their populations drastically reduced at about the same time.

If we leave aside the Cooke hypothesis and assume present rainfall conditions, it is by no means certain that the existing water sources and those which are known to have existed in the past — if properly developed and main-

tained — would not be adequate to supply the large population of Classic Maya times. It will be recalled that during most of the year there is plenty of water; the problem only becomes acute for three or four months in normal years. Reservoirs have been found at the two large ceremonial centers which have been most thoroughly explored, Uaxactún and Tikal. Two are reported at Uaxactún (Smith 1950: 84), seven at Tikal (Shook 1958: 18). *Aguadas*, both natural and artificial, are not infrequent near many areas of domestic settlement. It can be assumed that these have lost much of their storage capacity by silting and lack of maintenance in the more than a thousand years since abandonment. Some *aguadas* must have been obliterated entirely. The larger stream beds usually contain some pools which hold water long after the streams have stopped running and these could have been improved by damming or, in some places, by deepening of the beds. Permanent springs are occasional and the well near Dos Aguadas, described above, is probably not the only one of its kind. It is true that many of these sources are small and by themselves could not maintain for long a concentrated population comparable to a modern village or town. But the ancient Maya lived dispersed over much larger areas than is the custom today and, since they had no domestic animals, their per capita water demands were probably not so great.

Population and Agriculture

As part of an attempt to estimate population at Uaxactún, Ricketson laid out a cruciform area in the environs of the ceremonial groups which comprised 2,273,920 sq. m. of which some 953,040 sq. m. were habitable land not occupied by ceremonial ruins (Ricketson and Ricketson 1937: 15–24). Here were found 78 house mounds. Ricketson's map shows that he is counting separate platforms rather than the units of one to four platforms which in this report are called individual houses (Ricketson and Ricketson 1937, Fig. 2). Assuming five persons per mound, Ricketson derived a maximum population figure of 1083 persons per square mile (410 per sq. km.). Assuming only 25% of the mounds occupied simultaneously, a minimum figure of 270.83 per square mile was obtained. Ricketson goes on to assume that settlement of this density was uniform over the country and that these population figures would be applicable to the region as a whole.

Even higher estimates could be derived by similar means from data which we accumulated, although these are based on smaller samples. For examples, at Dos Aguadas we made a careful examination of a strip 200 m. wide along 2500 m. of a *brecha* made for petroleum exploration, an area of 500,000 sq. m. Not quite half of this area (215,000 sq. m.) was surveyed into more or less regularly spaced plots, in which we found 37 house ruins containing a total of 89 separate platform mounds. The remaining parts of the strip are predominantly unoccupied slopes so that the counted ruins, while not the total, represent well over half the total. Taking merely the 89 recorded platforms, we can, on the basis of five persons for each, derive a maximum figure of about 2300 persons per square mile (890 per sq. km.), or 575 per square mile if it is assumed that only 25% of the mounds were occupied at once. This is over double Ricketson's estimates.

But all such estimates must be regarded with the utmost skepticism. There is, of course, the problem of how many houses may have been occupied at once, and whether all the separate platforms associated with an individual house unit were ordinarily separate family dwellings. More important are the indications brought out by this survey that houses are not scattered evenly over the country but that they tend to concentrate, according to geography, in some areas at the expense of others. Thus a population estimate based solely on house mound counts within a particular area of ruins is no more representative of the true population per square mile of a region than, to take an extreme case, the population per square mile of Manhattan is representative of New York State.

The tendency for house ruins to concentrate means that there are tracts of potential farmland between house-ruin areas. Moreover, within house-ruin areas themselves there are usually large stretches of hill-slope which were not built upon and were very likely used for crops. Hillside locations are preferred for agriculture by the modern Maya of western British Honduras. According to A. C. S. Wright, former director of the Colony's soil survey, this is because hillside crops in a limestone country with clay soils and high and intense rainfall stand a better chance than crops in low-lying land.

The staple crop of the ancient Maya of Petén was presumably maize. It is likely that, as today,

this was grown by the *milpa* method of shifting cultivation, since no other practicable method is known ever to have been used in this tropical forest environment. The question has sometimes arisen whether the *milpa* method could have been consistently productive enough to have supported the inferred large population of the Classic period. A number of studies of *milpa* agriculture have been made in northern Yucatán (Steggerda 1941; Emerson and Kempton 1935; Hester 1953), but adequate data are still lacking for Petén, an ecologically somewhat different region with deeper soils and higher rainfall. Agricultural studies offer a promising method for estimating population limits, but too little is now known concerning *milpa* methods, soil potentials, and effects of *milpa* cultivation as these apply to the particular conditions of Petén. Whatever may be the exact answers, the abundance of house ruins throughout northeastern Petén makes it very easy to believe that overstrain of the agricultural potentials of the land by a too-large population was an important factor in the cultural breakup and abandonment.

Ruin Groupings and Settlement Organization

As has been pointed out, Maya settlement was in broad view dispersed, with the dispersion conditioned by geographical factors. But houses were not scattered as isolated homesteads each surrounded by its own farmland. Nor were they organized into compact discrete units comparable to true towns or cities, although houses ordinarily occur near other houses in what I have loosely called house-ruin "areas." In considering the specific groupings of house ruins and their relationships to ceremonial centers it is possible to recognize three main levels of settlement organization. For descriptive purposes these are referred to by terms which are not intended to carry definite socio-political connotations: Clusters, Zones, and Districts. The Districts are aggregates of Zones which are, in turn, aggregates of Clusters.

Clusters

In many localities where house ruins were abundant we noted a tendency for at least some of the houses to occur in clusters of about five to twelve within an area roughly 200 to 300 meters square or its equivalent, 4 to 9 hectares (Fig. 7). Often the clusters were separated from each other by features of terrain. For example, they were on the tops of ridges or on promontories of hills where areas of slope and dips in terrain separated them from other clusters. But clusters were also observed in places where there was no terrain break. In such places, the clusters were sometimes sufficiently separated so that definition was easy — for example, 200 or so meters apart. But others might be so close together that they in effect coalesced and it was more difficult to distinguish one cluster from another. The tendency for house ruins to occur in clusters could not be recognized everywhere, but it is likely that careful mapping of large enough stretches of territory would show that this type of grouping was more general than could be determined by simple traverses.

Sometimes one of the houses associated with a cluster is somewhat larger than the others or includes a structure which was probably a small shrine, but such architecturally distinguishable buildings are by no means a consistent trait of the clusters. Figure 2 *cc-ff* represents "houses" which are outstanding in the clusters with which they occur. Only one of these (Fig. 2 *ff*) would seem clearly to have been an exclusively ceremonial or public building. It was associated with an almost ideal example of the cluster type of grouping (Fig. 7 *a*).

The clusters may be thought of as small hamlets, varying in their degree of isolation from one another, and sometimes including an apparently specialized building which may have been used in common by the residents of the cluster. The size of the grouping suggests occupation by a kinship group.

Zones

Minor Ceremonial Centers can be found in nearly all areas where house ruins are abundant. The frequency with which Minor Centers are encountered is clearly in direct proportion to the abundance and density of house ruins in the neighborhood. An over-all estimate — for which no great accuracy is claimed — is that there is a Minor Ceremonial Center for every 50 to 100 houses on the average. The area containing these houses which are presumably associated with a Minor Center is a "zone" of settlement. The smaller clusters of houses which were described above are subdivisions of the zones.

Some zones of settlement occur by themselves

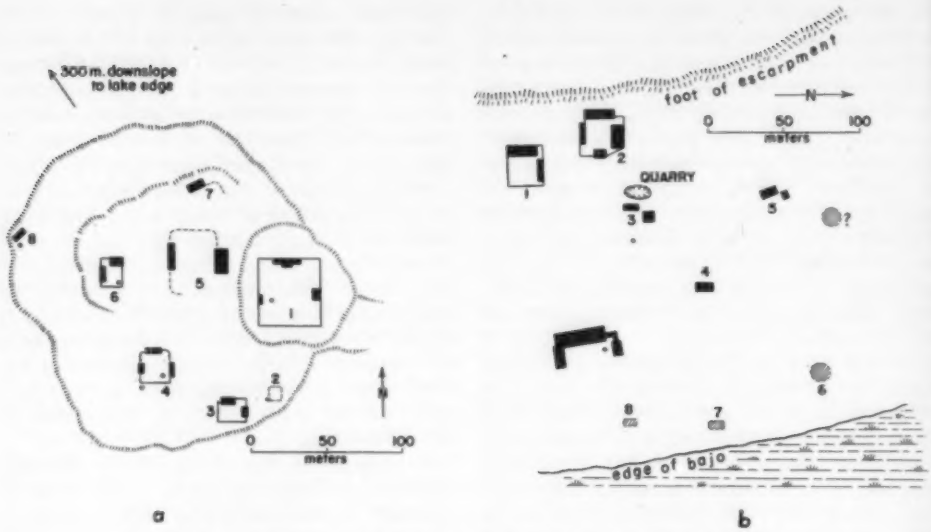

Fig. 7. Examples of house ruin clusters. *a*, Yaxhá, Area E; *b*, Dos Aguadas, Area A.

in isolation from other zones. For example, they may be in the vicinities of isolated *aguadas* or on isolated hill-masses or ridges. In other places, there is no definable break in house-ruin distribution between one zone and another and the existence of the zones can only be recognized by the spacing of the Minor Centers and their consistent occurrence with about every 50 to 100 house ruins. The amount of area covered by the houses of any particular zone varies according to the local topography and density of settlement. My observations suggest that a rough average would be about one square kilometer, but zones in which the topography was irregular and the houses more scattered occupy a greater amount of space.

Within a zone, the Minor Ceremonial Center does not necessarily occupy a geographically central location, nor is there a consistent tendency for the houses to occur in greater density near the Minor Center than further away. In fact, the centers are often comparatively isolated by their positions, which are characteristically on hill summits or other prominent places.

The inference is drawn that zones of settlement were significant community units, the Minor Center having served as the religious and civic center for the community. They may be compared in a general way to dispersed towns or townships.

Districts

As Minor Ceremonial Centers are the nuclei, in a functional sense, for zones of settlement, Major Ceremonial Centers are similarly nuclei for larger units of settlement called "districts." The zones are subdivisions of the districts.

The distribution of house ruins within a district and their locations relative to the Major Center vary according to the local geography. In some districts house ruins are abundant in the vicinity of the center, but in others they are more abundant in other parts of the district and settlement near the Major Center is comparatively light. To cite specific examples, the Major Ceremonial Center of Yaxhá overlooks a large lake with more than 30 kilometers of shore frontage. The density and abundance of house ruins close to this large ceremonial center are no greater than elsewhere on suitable terrain around the lake; in fact, the greatest densities are on the opposite side of the lake. The ruin of Holmul occupies a broad hilltop about a kilometer from the Holmul River. The densities of house ruins were found to be appreciably greater along parts of the river and especially along the edges of the Bajo de la Chamaca, which lies south of Holmul, than they were about the ceremonial center itself. On the other hand, settlement is dense in sectors close to the

Major Ceremonial Centers of San Clemente (El Gallo) and Dos Aguadas.

As was true to a lesser extent for the Minor Centers, the specific position of a Major Center is often responsible for its comparative isolation. The Major Centers typically crown hilltops or other lofty positions from which they could be seen for miles if the forest were cut, and these locations did not necessarily offer the most practicable sites for domestic residence. It may be concluded that, whereas nearness to water supply and perhaps to good farmland were important determinants for domestic settlement, Major Ceremonial Center locations were more influenced by prominent topographical position and probably accessibility from all parts of their subordinate districts.

Most, if not all, of the house ruins belonging to a district are organized into zones, each of which had its own Minor Ceremonial Center. Minor Ceremonial Centers may be located quite close to Major Centers — within about one kilometer — if domestic settlement near the Major Center happens to be comparatively heavy. But if domestic settlement near the Major Center is light, the Minor Centers will be found in the more distant areas where house ruins are more abundant. The more intimate relationship which was observed between the Minor Centers and the house ruins is an important indication of differences in function between the two classes of ceremonial centers. Each separate zone of settlement, regardless of how far it may have been located from a Major Center, had its own ceremonial and civic buildings, while the Major Center was evidently associated with no particular zone of settlement but must have served in common the inhabitants of all of the various zones within its district. Another indication of a functional difference between Major and Minor ceremonial centers, other than comparative size, is the customary presence in Major Centers and absence in Minor Centers of stelae and ball courts. In some specific instances where Major and Minor centers occur near each other it is difficult or impossible to distinguish between Minor Centers and architecturally independent outlying groups of a Major Center. The former typically contain smaller and less elaborate buildings than the latter and are arranged more compactly, but these criteria cannot be counted upon and interpretation of function must be based on knowledge of the over-all settlement pattern in the vicinity.

Both Morley (1946: 313) and J. E. S. Thompson (1954: 67) have expressed belief that, in Thompson's words, "the Maya nobility almost surely resided on the outskirts of the city, and the peasant and working population lived farther away in small scattered settlements." Judging by the size and appearance of house ruins, the survey failed to find evidence for a concentric social gradation of this sort. It is true that comparatively large house ruins can be found in the environs of Major Centers, but it is also true that equally large house ruins occur in settlements far from the Major Centers. No obvious over-all regularities in the location of large and small houses relative to each other could be found, although this is a subject which further work might investigate in more detail. In some localities the houses on more elevated positions such as the crests of ridges tend to be slightly larger than those on lower ground, but in other localities large and small houses occur together. It is reasonable to suppose that Major Ceremonial Centers had cadres of priests, administrators, and others. It seems probable that these lived in the often enormous palace-type buildings which are usual features of the Major Centers themselves. But it also seems probable that many of the Maya leaders lived scattered among the rest of the population perhaps as a sort of rural nobility.

I shall not attempt to estimate the number of houses or of zones which were subordinate to a Major Center. Major Centers vary greatly in size and presumably their populations did also. The distribution of known Major Centers in northeastern Petén suggests that the average district comprised slightly less than 100 sq. km. of upland, a figure which would have to include farmland as well as areas actually occupied by ruins. But some districts were undoubtedly a great deal more extensive than others. Some districts are more or less isolated by geography. For example the settlement pertaining to the ruin of Dos Aguadas is mainly limited to a stretch of country lying between two *bajos*, each several miles across. In other places there appears to be no definable break between the settlement of one district and that of another, the situation in this regard being comparable on a larger scale to what was described for the zones and clusters.

The districts may be likened in a general way to counties or provinces, each with a Major Ceremonial Center as its functional, but not necessarily geographical nucleus, and each subdivided into township-like zones which vary in their degree of isolation from one another.

As for the relationships between Major Ceremonial Centers, it must remain conjectural whether a particular center was larger than another because it was the capital of a larger and more populous district, or because it was the capital of a larger region which included several districts, each with a Major Center of smaller size. It is not unreasonable to suppose that an especially large ceremonial center such as Tikal was at least a religious and pilgrimage center for a large area and it is quite possible that it bore the same relationship to certain other Major Centers and their districts as these bore to their subordinate Minor Centers and Zones.

Summary and Comments

The picture that emerges is of a dispersed type of domestic settlement with the dispersion conditioned principally by the availability of water supply and suitably large areas of level, well-drained house sites. We can recognize three main levels of settlement organization. First, there is the tendency for houses to occur in clusters of about five to a dozen, sometimes with an apparently specialized building which probably served a community function for the cluster. Second, there are aggregates of clusters composing zones of perhaps 50 to 100 houses, each with a Minor Ceremonial Center which served ceremonial and civic functions for the houses of its zone. Third, there are aggregates of zones composing large districts, each with a Major Ceremonial Center of district-wide religious and civic function. The Major Centers are not only appreciably larger than the Minor Centers, but they contain stelae and other specialized features which the Minor Centers do not ordinarily have. The clusters may be likened in a general way to hamlets, the zones to dispersed towns or townships, and the districts to counties or provinces.

The clusters, zones, and districts may be isolated from others of their respective categories, or settlement may be more or less continuous between them. In this latter respect, the pattern is somewhat similar to the modern phenomenon seen around industrial centers where, with expansion of population, the formerly separate cities and towns tend to coalesce so that there is no break in the settlement between them, although each retains its own political and civic identity. A program of carefully selected test excavations could probably determine whether the apparent coalescence of settlement between Maya communities was also the result of expansion due to population growth.

In a general way, settlement in northeastern Petén resembles that found by the survey directed by Willey along the Belize River in western British Honduras. The principal differences result from the particular river conditions of the latter region which are absent in the area of this survey. In British Honduras, Maya settlement concentrates heavily in a practically continuous strip along the flatlands bordering the large permanent river. Ceremonial ruins comparable to the Major and Minor centers of Petén occur at intervals along the river, usually on bluffs overlooking the valley. House mound concentrations near the ceremonial ruins are no greater than elsewhere along the river. Level land offering good residential sites stretches along the Belize River almost without break, and it is more difficult there than in Petén to separate the continuum of house mounds into segments corresponding to community units.

Topoxté, a Postclassic Settlement

Only settlements of the Classic period have been considered so far in this paper. On a group of islands in Lake Yaxhá is a community that is almost surely Postclassic in age. On one of the islands is a small ceremonial group known as Topoxté (Maler 1908: 55–60; Lundell 1934: 182–5). The island in question is about 400 by 200 m. across. Exclusive of the ceremonial group of 10 or 12 buildings, there are between 80 and 100 house platforms crowded onto the little island. Most are on large terraces made to provide level places on the irregular rocky ground of the island. Construction occurs almost everywhere to within about 4 m. above the present lake surface. Two nearby islands are likewise crowded with house structures. We have here a very different type of settlement than prevailed in the same area during earlier times: a true town or small city of at least 200 houses clustered tightly about a ceremonial center on a group of islands.

Topoxté recalls conquest-period descriptions

of the Itzá capital of Tayasal, which was located on a group of islands in Lake Petén. It is possible that the occupation of the two communities overlapped, but almost total lack of knowledge concerning the Postclassic sequence in Petén makes definite comparisons impossible at this time. Estimate of the late date of Topoxté is based on the standing architectural remains and on pottery collections taken mostly from the surface. Previous visitors to the island have not commented on the obvious architectural contrasts between Topoxté and the nearby ruin of Yaxhá, which is a typical Classic Petén ceremonial center. Striking features of Topoxté include absence of vaulted buildings, presence of columns — some of which are made of circular drums — in doorways, stairway balustrades with vertical upper zones, and small, almost miniature, stelae formerly ornamented with stucco. Important ceramic traits include human effigy incensarios not dissimilar to those found at Mayapán and at other late sites in the north part of the Yucatecan peninsula, and vessel foot types which resemble those found in late sites on the Yucatecan east coast. These are ceramic features which are also present in miscellaneous collections from Ciudad Flores, which stands on the site of the Itzá capital, and from a few other locations in Petén and British Honduras. Other sherds from Topoxté indicate that the island was also occupied in Late Formative and Classic times. A separate report on the ruin of Topoxté is planned.

Concluding Remarks

Numerous observations made over large stretches of country in northeastern Petén have made possible a characterization of settlement in an important region of Classic Maya culture in terms of the types of ruins, their distributions, and their relationships. Although emphasis has been placed on determining the general over-all pattern, it is recognized that variations in this general pattern will occur in certain particular localities. The findings of the survey suggest specific topics for more intensive investigation. Among these may be mentioned especially the need for increased definition of the "cluster," "zone," and "district" types of groupings, their variations, and their beginnings and growth. It is suggested that this could be accomplished by a program directed towards (1) the systematic exploration of a selected area of district size in order to determine as well as possible the locations and numbers of house-ruin areas and Minor Ceremonial Centers, (2) the mapping of selected areas of zone size in order to bring out more exactly the numbers of houses subordinate to a Minor Center and their clustering, and (3) test excavating of selected house ruins in order to date their first construction and length of occupation. Such test excavating could throw light on population changes, the growth and suggested coalescence of ruin groupings, possible settlement shifts related to changes in water supply, and similar problems. With such information we shall have a firm body of data for evaluating the social and economic basis for the growth and decline of Classic Maya civilization in Petén and also for evaluating the hypotheses which have been advanced for explaining this growth and decline.

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THE FREMONT CULTURE: INTERNAL DIMENSIONS AND EXTERNAL RELATIONSHIPS

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ABSTRACT

The Fremont culture of northeastern Utah is predominately Anasazi in character, but has a few distinctive traits. It exhibits great variability, especially in ceramics and architecture. The total time span of this complex may be the 250-year period from A.D. 950 to 1200. The Fremont culture, and probably the very similar Sevier culture of western Utah, represent a rather sudden northward movement of traits, and perhaps people, from the independently-developed, but Kayenta-influenced Virgin branch of the Anasazi.

THE CULTURAL POSITION of Pueblo and Pueblo-like complexes in Utah north of the Colorado River has long been uncertain. While this problem cannot yet be completely solved, data obtained from a recent extensive survey of the Fremont area and from work in the Glen Canyon, added to information already available, make possible new interpretations of the archaeology of the region which may provide a fruitful point of departure for further work. This paper is a discussion of the internal dimensions of the Fremont culture, and of the probable relationship of Fremont to Anasazi. It suggests specifically that most if not all of the Pueblo traits in Fremont originated in the as yet poorly defined subdivision of Anasazi for which Colton (1942: 36, 1955: 5) has suggested the name "Virgin Branch."

The Fremont complex has been recognized as distinct from Anasazi culture since 1931 when Morss reported his work along the Fremont River in southern Utah. Subsequently, the Fremont culture has received an increasing amount of attention. Gillin (1938) carried out excavations in Nine Mile Canyon. Work at Fremont sites in the Yampa Canyon in the extreme northwest part of Colorado has been reported by Burgh and Scoggin (1948) and Lister (1951). Wormington (1955) has reported several years' work at a site near Cisco, Utah, and summarized the Fremont culture as of that time. Between 1954 and 1957 the University of Utah carried out a survey of the Fremont area with limited testing (Gunnerson 1957) and excavated two sites (Taylor 1957). Subsequent reconnaissance (Lister 1958) and excavation (Gunnerson 1959) south of the Fremont area contributed data which suggested the Fremont–Virgin branch relationship outlined here.

INTERNAL DIMENSIONS

The Fremont culture is still not really well known, but there is already ample evidence of great variation in Fremont ceramics, architecture, and stone work (Gunnerson 1956, 1957). Fremont may be said to be characterized by experimentation and a variability especially interesting in that it is nearly as great within many sites as between sites. There are, however, generalized patterns and specific traits which occur consistently from site to site. Still other traits seem to vary in frequency across the area, especially from north to south.

Considering first those traits which appear consistently throughout the entire area, we find the following. Villages are never large. A typical site would have from about six to 12 houses or rooms, although many sites appear to have had only one or two houses. In the case of a few large sites it is difficult to decide whether there were several small villages close together, or one larger village of scattered houses. The favored location for sites is on low knolls or ridges adjacent to lower level land suitable for horticulture. Many of the sites are along small, often intermittent streams, or where the streams emerge from mountain canyons into near-desert land that was probably irrigable by simple diversion of flood water. Throughout the entire Fremont area use was made of caves and rock shelters; some open camp sites in wooded, sandy areas may have been occupied by the Fremont people, although diagnostic artifacts are seldom found in such places. Also, there is considerable homogeneity among the pictographs and petroglyphs of the region and certain distinctive elaborate anthropomorphic designs are generally attributed to the Fremont people, although most of the design elements found are not restricted to this area. Subsistence economy was based upon corn, bean and squash agriculture. Bones of game animals are found but are not usually numerous.

The Fremont traits with which we are best acquainted are the pottery traits because much of our information is from surface survey. Fre-

mont pottery may be divided into two main types on the basis of tempering and these types can be correlated with geographical areas. The northern part of the Fremont area, that from the foothills of the Uintah Mountains south to the Book Cliffs, is characterized by plain gray pottery tempered with crushed calcite. That part of the Fremont area south and west of the Book Cliffs is characterized by plain gray pottery tempered with crushed igneous rock. A few sites near the Book Cliffs have yielded both types.

Wormington (1955: 165) feels that because calcite is a poor tempering material and difficult to obtain, its use was culturally conditioned. It seems at least equally probable that the type of tempering material used by Fremont people reflected the type of material available. In the area of calcite tempering there is practically no igneous rock, but where igneous rock was used, calcite is also available. This suggests that calcite was used as a substitute where igneous rock was not available and that the basic trait was the crushing of rock for tempering.

In addition to the basic difference in tempering, there are numerous variations in decoration. The calcite-tempered pottery is seldom decorated and is apparently never painted. The majority of sherds tempered with igneous rock also have a smooth, plain outer surface, but many are decorated by painting and tooling, and a few are corrugated. Both mineral and organic black paint were used on both slipped and unslipped surfaces. Designs are almost always geometric, composed of straight and curved lines of various widths, and dots. For the most part, painting is moderately well executed and designs most closely resemble Black Mesa, Sosi, and Dogoszhi styles and combinations of these styles, which are commonly found in the Kayenta and Virgin branches of the Anasazi.

The greater number of pottery sites in the southern part of the Fremont area, and the greater elaboration of pottery there, may reflect the proximity of the southern Fremont people to the Anasazi. Perhaps, however, the technical inferiority of the calcite-tempered ware discouraged its makers from producing it in quantity, and from decorating it.

A major decorative device characteristic of Fremont pottery was the manipulation of the smoothed surface before firing. Such decoration included incised lines, lines apparently formed of finger-nail jabs, and areas filled with lines of punctation made with finger nails or other tools, such as sticks. Incised lines often take the form of scoring, with one set of lines vertical on the neck of vessels and the other horizontal. Surface manipulation is almost always restricted to the necks and shoulders of vessels but sometimes occurs on the handles, which usually extend from lip to shoulder. Nodes and appliquéd punched pellets (coffee-bean appliqué) are other decorative devices used. Rarely, incised decorations are found on the vessel lip. Although corrugation was accepted by the Fremont people only to a very limited extent, and corrugated sherds are not common at Fremont sites, those that occur represent a wide variety. Plain corrugated sherds exhibit corrugations ranging from narrow to broad. Indented corrugated sherds are also found, but the elaborate decorative designs formed by combining plain and indented corrugation or corrugation and incising, or panels of pinching on otherwise plain corrugated vessels have not yet been noted. A very few sherds show painting on the inside and corrugation on the outside and a few sherds referred to as "false corrugated" appear to have been incised or tooled to resemble corrugated ware. Excavated Fremont pottery sometimes shows a thin fugitive red layer on the outside.

In addition to pottery vessels, Fremont people also made pottery elbow pipes and elaborate unfired clay figurines. Sherds were reworked to form discs varying in size from about 1.5 to 4.0 cm., and some were perforated, presumably for ornaments and spindle whorls. Unperforated discs were probably gaming pieces. Other worked sherds are of various sizes and shapes and many were probably used to scrape pottery.

The variety of decorations found on the technically superior igneous rock-tempered ware suggests importation of pottery styles, some of which were elaborated by the Fremont people. Actual trade sherds, however, are rare at most Fremont sites, those that occur being from southwest Utah or northern Arizona.

Nonceramic artifacts are less useful in establishing relationships between Fremont sites and between Fremont and other complexes. Projectile points and blades show wide variation and are not common from surface collections since many of the sites have been picked over by col-

lectors. Metates, which vary from deep basins to nearly flat milling stones, are somewhat more helpful. One type, characteristic of central Utah, is the so-called "Utah metate," which has a large, deep trough at one end and a smaller depression or shelf at the other. Manos from the foothills of the Uintah Mountains often show a characteristic rocker shape. Both one-hand, slightly oval manos and loaf-shaped, two-hand manos are found over the whole area.

Small ground stone artifacts include turquoise pendants and discs of other soft stone which are sometimes perforated. Ornaments were also made of bone, elk teeth, and bone carved to resemble elk teeth. Rectangular bone gaming pieces with various forms of markings are frequently found. Several forms of awls, generally well finished, are the most common bone tool. Some have all or part of the articular end remaining. Others are nearly cylindrical, and still others were made from bone splinters. The "Fremont moccasin" with dew claw hobnails, which seems to be exclusively a Fremont trait, is found in the Yampa-Uintah sites as well as in the Fremont River drainage.

Architecture shows variations which do not appear to be correlated with chronology. In the Fremont area there are several types of structures which have little or no cultural material associated with them. The most spectacular are the so-called forts or towers, consisting of one or a few rooms of masonry, probably dry-laid, located on high pinnacles, ridges, or canyon edges. Many of these are several hundred feet above the valley floors. It seems unlikely that they were residences, but some may have been. More plausible explanations are that they were storage rooms or were ceremonial in function. Their identification as Fremont is based primarily upon their location in areas where the other archaeological remains are usually Fremont.

Structures commonly attributed to the Fremont people are small storage rooms built in rock shelters or under overhangs, often where they are well concealed and difficult to get to. Construction is of stone and mud, or of adobe "turtle backs." Design seems to be dictated more by the nature of the rock shelter than by other considerations. Often all or a portion of a niche or rock shelter is walled off to form a cist. Some rock shelters have dwelling rooms, or have served as habitations.

The structures in the open villages vary greatly. There were surface houses and pit houses, both square and round. Walls were of adobe, adobe and stone haphazardly combined, stone slab masonry, boulder and adobe masonry, and jacal, or at least poles and adobe. Masonry or perishable walls were apparently built on boulder foundations in some instances. Most structures were single rooms, but some multi-room structures are known. Walls of pit houses were either unplastered, plastered with adobe, or lined with boulders. Benches are not found. Floors were often plastered with adobe. Post holes were sometimes in a regular pattern of four, sometimes randomly scattered and sometimes missing. Usually in dwellings there was a central adobe-rimmed fireplace, often paved with stone slabs. Both living and storage rooms are found at villages; sometimes surface granaries are associated with pit houses. Outside firepits sometimes occur.

Only additional excavation will furnish the data necessary to determine whether there has been any sequence of development of Fremont house types. It appears at present that, as in the case of ceramics, many types of houses were in use at the same time.

The skeletal material from Fremont sites is still inadequate for a definitive study. A few traits are of interest, however. Most of the skulls assignable to Fremont sites show marked lambdoidal flattening and would not be out of place in a Pueblo series. In the small sample, dental pathologies and exostoses, on vertebrae especially, are quite common (Gunnerson 1957: 31–4).

Very few tree-ring dates are available for Fremont sites and many of those are questionable. Schulman (1948) and Smiley (1951) record several dates from sites, probably Fremont, in Nine Mile Canyon, which is located about midway north and south in the Fremont area. The outer rings of the samples dated with confidence range from A.D. 915 to 1145. Most of these samples had many outer rings missing. The only specimen with bark present was the one with the 1145 date; even this had crowded outer rings and was thought by Schulman to have some rings absent. Smiley (1951) considers the dating of only one Nine Mile Canyon site, NM–13, "good." For this site he reports three dates between A.D. 1055 and 1061 and six dates between A.D. 1078 and 1090, but all specimens have rings missing.

Two sites in Hill Creek Canyon, just across the Green River to the east of Nine Mile Canyon, have also been dated. Four samples of charcoal from one site, all with many rings missing, gave dates from A.D. 957 to 1073 (Schulman 1948). Smiley (1951) includes a date of A.D. 1000 for a second site in Hill Creek Canyon and evaluates the site dating as poor. The earliest date suggested comes from the Yampa valley in northwest Colorado, where an estimated cutting date for a house timber is provisionally given by Schulman (1950) at A.D. 750±50 years.

Cross dating of Fremont sites by means of trade pottery has extended the time range toward the more recent. A few sherds of Tusayan Polychrome have been found at Fremont sites in the Castle valley area. This type has been dated by Colton and Hargrave (1937) at A.D. 1150–1300. Tusayan Black-on-red sherds have also been found and are datable at A.D. 850 to 1125.

Taylor (1957: 49, 96) identifies sherds from both 42Sv7 and 42Sv21 as Tusayan Black-on-white. If this identification is correct, these sherds would suggest that the sites were occupied until sometime between A.D. 1225 and 1300, the period during which this type was made (Colton 1955). A re-examination of the pottery from these two sites, however, failed to confirm the Tusayan Black-on-white identification for any sherds. Most of the sherds so identified by Taylor are probably of local manufacture.

The locally made black-on-white and black-on-gray pottery shows designs of Black Mesa, Sosi, and Dogozshi styles. The black-on-white types which give these decoration styles their names are found primarily in northern Arizona and are dated respectively at approximately A.D. 900–1100, 1070–1150, and 1070–1150 (Colton 1955). There is a possibility that these styles persisted longer in the Fremont area than farther south, but it is highly improbable that they occurred earlier in the Fremont area.

On the basis of present evidence, then, it seems advisable to date Fremont culture at about A.D. 950 to 1200, for the best tree-ring dates fall for the most part in that period, and dates before about A.D. 1000 are questionable. Moreover, the trade pottery supports an A.D. 950–1200 range for Fremont sites in the middle and southern parts of the area.

Not only was Fremont contemporaneous with the Developmental Pueblo period in the Anasazi area, but most of the sites for which dates are available appear to fall in late Developmental Pueblo times. Even the earliest tree-ring date, which since it is estimated and provisional may well be too early, is still post-Basketmaker.

Anasazi Relationships

Steward (1940: 468) felt that the essential core of the culture of the "Northern Periphery," including the Fremont culture, was derived from the Anasazi. More recently, however, it has been suggested that the Fremont culture developed in northeastern Utah out of the hunting and gathering Desert culture (Jennings 1956: 102–4; Taylor 1957: 146–8). At no site has this development been clearly demonstrated. Perhaps the site that has the best sequence is Hells Midden, located on the Yampa River in the extreme northeastern corner of the Fremont area, where the culture is most attenuated. Here Lister (1951) excavated a 5-meter thick midden deposit. In the second level from the top and in the upper part of the third level from the top (there were 15 levels altogether) he found calcite-tempered pottery (Fremont) associated with evidence of maize and an artifact inventory which differed significantly from that found below. During the occupation of the third level from the top there was apparently an abrupt shift to a horticultural way of life with the introduction of the Fremont culture.

Hunt (1953) has identified many sites in the LaSal Mountain area as non-pottery Fremont, early Fremont, and Fremont (?). Her evidence for most of these identifications is tenuous. Similarly, Rudy (1954) at one time classified several sites in Beef Basin, northeastern San Juan County, as Fremont, but a re-examination of the field notes and specimens disclosed that these sites were characterized primarily by an absence of pottery or other diagnostic artifacts. It would seem best to assign all of these nonceramic sites to the Desert culture as defined by Jennings and Norbeck (1955: 2–4). During the archaeological survey of the Fremont area (Gunnerson 1957), a number of nonceramic sites, probably Desert culture, were found which could be ancestral Fremont, but there is no strong evidence to support such an assignment.

Until much more evidence is available, the term Fremont should be restricted to sites comparable in content to those in the Fremont drainage described by Morss (1931). This would exclude non-ceramic sites. At any rate, the discovery, during the 1954–56 survey, of numerous Fremont sites with substantial dwellings and relatively large quantities of painted pottery with Anasazi-influenced designs has turned attention again to the Anasazi region as the probable source for much of Fremont culture, and has helped to make the relationship between the Fremont culture and the Anasazi development in general somewhat clearer.

Thus far, we can distinguish a Fremont culture as it existed for perhaps two and a half centuries, prior to about A.D. 1200. For most Fremont traits one can find specific counterparts in the Anasazi culture at the same or earlier times, and few traits which made their appearance in Anasazi culture before or during Pueblo II are lacking in the Fremont culture. Differences for the most part are ones of quantity or relative importance. The most spectacular trait found at *some* Developmental Pueblo sites, but missing at Fremont sites, is the large multi-room pueblos. Nowhere in the Fremont area is there to be found evidence of large concentrations of population at single sites. Thus far there have been no kivas reported from controlled excavations, but the description of one structure excavated by an amateur on his own property near Ferron, Utah, is suggestive of a kiva. The small amount of excavation completed may account for the lack of evidence of kivas. Turkeys, also, are thus far unreported. Stone axes are apparently missing in the Fremont complex, but these are also scarce in Developmental Pueblo complexes. Corrugated pottery was known to the Fremont people as a trade item (Tusayan Corrugated) and they made a very little locally, but they apparently chose to make more than 99% of their utility ware plain, or plain with tooled decorations.

Direct comparison with Anasazi of the Four Corners area has made the Fremont culture seem an anomaly of late Developmental Pueblo times; for in Fremont, Basketmaker traits such as plain utility ware, pit houses, and clay figurines, nearly extinct in the Four Corners area in Pueblo times, occurred together with Pueblo traits. Hence Steward felt that a culture ancestral to the complexes of the "Northern Periphery," and showing the same blend of Basketmaker and Pueblo traits must exist somewhere between the San Juan area and the Northern Periphery. He searched specifically for an archaeological complex having the requisite traits, only to conclude that the region east of Kanab was not related to the "Northern Periphery," and that the source of "Northern Periphery" culture should be sought in western Colorado or extreme eastern Utah, then an archaeologically unknown area (Steward 1941: 281–2, 355).

A review of the results of Steward's work east of Kanab plus additional information from the northern drainages of Glen Canyon (Lister 1958, Gunnerson 1959) has revealed that sites as far east as those in the Escalante drainage can be at least tentatively assigned to the Virgin branch of the Anasazi. Furthermore, this work along with information now available from the Zion Park area west of Kanab (Schroeder 1955) strongly suggests that the Virgin branch contains at a relatively late time a complex of early and late traits from which the Pueblo traits in Fremont could have been derived.

The Virgin branch, in brief, is thought to include the pueblo sites in the northern drainages of the Colorado River from the Escalante River drainage on the east to, and including, the Muddy River on the west. This branch appears to have developed independently, but with strong Kayenta influence, from a Basketmaker level through a Classic Pueblo level. However, a few traits which characterize the better known branches of Anasazi, especially in the Pueblo I stage, are lacking in the Virgin branch, although some of the differences may only reflect the uneveness of the data. The archaeology of the eastern part of the Virgin area, for which the name Zion subarea has been suggested, and which is contiguous to the Fremont area, is best known from the work of Schroeder (1955) in Zion Park, Steward (1941) in the Paria–Johnson Canyon area, and Rudy and Stirland (1950) in Washington County. Colton (1952) describes several pottery types found in this area.

It is postulated that the Fremont culture of eastern Utah, and probably the very similar Sevier culture (called "Sevier-Fremont," by Jennings 1956: 104; Taylor 1957: 162) of western Utah, represents a northward expansion of Anasazi culture from the Virgin branch after

about A.D. 950 and before A.D. 1200, the approximate terminal date of Fremont. It seems probable that the distinctive flavor of the Fremont culture, however, resulted from a blending of Virgin traits with the Desert culture traits of a sparse indigenous population.

A number of traits which distinguish the Virgin branch from other Anasazi complexes could have been the source for similar Fremont (and Sevier) traits. One of the most characteristic traits of the Fremont culture is the predominance of plain utility ware as opposed to the corrugated wares which were then in vogue in much of the rest of the Anasazi area. In the Virgin area, plain pottery was introduced in an essentially Basketmaker III context, and persisted into Pueblo III times. The Four Corners Pueblo I pottery, with the start of corrugation in the form of neck banding, appears never to have occurred in the Virgin area in any significant amount. Virgin pottery shows a gradual transition into a complex comparable to that of the Pueblo II farther east, except that plain utility were continued to predominate over corrugated wares until Pueblo III times.

Not only does Fremont pottery appear to have developed from a Virgin base, but it seems to have its genesis in the later Pueblo II Virgin pottery. Schroeder (1955:103) states that plain ware associated with the Basketmaker III through early Pueblo II painted types "usually contains fairly large subangular quartz temper in relatively clean paste" and that the plain ware associated with late Pueblo II and early Pueblo III types "usually contains small quartz, with occasional large particles and varying proportions of fragments which sometimes dominate," and that associated decorated pottery and associated trade sherds never show the large rounded quartz particles found in the earlier pottery from the Virgin area.

Of the various regions of the Virgin branch, the Escalante drainage seems to show the largest proportion of angular fragments with the sand tempering. If this tendency is carried further one gets the crushed igneous rock tempering of Fremont pottery (Gunnerson 1956) or the abundance of moderately fine to fine, angular sand tempering derived from igneous rock, with very sparse quartz sand, found in Snake Valley and Sevier Gray pottery of western Utah (Rudy 1953: 83–91). Steward (1941: 302) noted similarities between the plain gray pottery of the Paria and Johnson Canyon region and Sevier Gray. The common occurrence of dots in lines or filling areas on Fremont painted wares has been somewhat puzzling, since dots are most commonly found on Lino Black-on-gray pottery dated earlier than A.D. 750. But Virgin pottery also made use of dots in designs, and this may have been the source of the Fremont trait.

Relationship between the Fremont culture and the area to the south is further supported by the presence at Fremont sites of Kayenta, or Kayenta *affinis* (Virgin) trade pottery and the absence of trade sherds from the Four Corners area. A re-examination of pottery from the Old Woman and Poplar Knob has failed to disclose any Mancos Black-on-white sherds as reported by Taylor (1957: 49, 96). The sherds so identified are of local manufacture and can be assigned to the type Turner Gray-Variety Two (Wormington 1955: 71–3; Gunnerson 1956).

Several other distinctive traits found in both Fremont and the Virgin branch suggest a relationship between the two complexes. Both had a variety of styles of manos and metates, including the distinctive Utah metate. Skin moccasins, while not of the usual Fremont type, have been found along with sandals at one Virgin site. Stone balls are not as common in Virgin sites as in Fremont, but they do occur. Clay figurines, a hallmark of Fremont culture, occur in Virgin sites.

The sharing of several architectural traits also suggests a relationship between the two complexes. Both utilized simultaneously a variety of types of construction: sometimes, for example, within single structures one wall may be of masonry and the other three of perishable materials. Small storage bins or granaries are commonly found in small rock shelters in both areas. Several architectural traits found elsewhere in Anasazi are lacking in both Virgin and Fremont. Neither group has large, multiroom pueblos. Neither group carefully shaped and dressed stone slabs for building. Double walls with rubble cores were not constructed. Common pit-house features missing in both are multiple rooms, vestibules, and partitions.

The Fremont settlement pattern was very similar to that of the Virgin branch. Sites were small; those containing a dozen or more rooms were rare. In especially desirable areas, sites were sometimes situated within less than a tenth of a mile of one another. Combinations

of different types of structures at a site, such as surface granaries associated with pit houses, are found in both complexes. Open sites are usually found on ridges or knolls near tillable land and sources of water.

Two generalizations can be drawn which suggest that the basic orientations of the two cultures were similar. Both appear to be characterized by a great deal of variability, with many alternative traits. This in turn may be the result of a tendency, most obvious in the Virgin branch because of its greater time depth, for old traits to be retained rather than discarded when new traits were added to the complex.

SUMMARY

It is suggested as a hypothesis that the Fremont culture (and probably the very similar Sevier culture) represents a northern extension of many Pueblo traits and perhaps people into northeastern (and northwestern) Utah on a Pueblo II-early Pueblo III time level, and that these traits stemmed from the poorly known Virgin branch of the Anasazi which appears to have developed independently, but with very heavy Kayenta influence in the northern drainages of the Colorado River from that of the Escalante River on the east to and including that of the Muddy River on the west.

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A PRELIMINARY REPORT ON PETROGLYPHS IN PUERTO RICO

MONICA FLAHERTY FRASSETTO

ABSTRACT

Antillean petroglyphic art reached its highest development in the technical excellence and lively artistic expression of the many petroglyphs in Puerto Rico. This report is the beginning of a systematic study of Puerto Rican and Antillean petroglyphs which is intended to trace stylistic developments through the Antilles to South American points of origin. Sixty surface prints were made at 13 sites in three regions of Puerto Rico, but only those from Salto Arriba, Icacos, and Caonillas are discussed in detail. The simple and accurate, though time-consuming method of surface printing is fully explained. There are two distinct local types or styles of petroglyphs in addition to the simple curvilinear and abstract designs. The swaddled infant type appears at least as early as Rouse's Period III and represents the beginning of a local petroglyph style. The later Capá type reaches its final stage during Period IV. This study confirms Rouse's placement of the Puerto Rican petroglyphs in his Periods III and IV (A.D. 350–1584), and demonstrates the development of local petroglyph styles parallel to the development and spread of Taino culture.

ALTHOUGH considerable material has been published about the archaeology of Puerto Rico, as yet no independent systematic study has been devoted to the petroglyphs of that island. These rock carvings are found on river boulders, walls of caves and rock shelters, and on upright stone slabs associated with aboriginal "*juegos de bola*" or dance plazas. Unfortunately many of the latter have been carried off by collectors. Caves have also been rifled. Sites being abundant, however, there is still plenty of undisturbed material for the student. In the Antilles, Puerto Rico is the place where petroglyphic art reached its highest development. Fewkes (1903–04: 150) sets it at a higher technical level than the petroglyphic art of North America, north of Mexico.

Technical excellence and lively artistic expression, however, do not enable us to decipher the meaning of these theriomorphic and "abtract" symbols. We are handicapped by the absence of historical reference or legend concerning them. In South America, for instance, the Indians could not (or would not) tell early European explorers who made the petroglyphs there or what they meant, saying either that they were the work of the "Great Spirit" or shrugging them off as the handiwork of women (Schomburgk 1841: 147; Im Thurn 1883: 402).

In Puerto Rico, we can at least hope to trace certain stylistic developments as (and if) they occur in relation to other Antillean areas and points of origin in South America. Assuming the correctness of Rouse's dating of the Puerto Rican petroglyphs (Rouse 1949: 501–2; Cruxent and Rouse 1958) to within the last two periods of his time scale for Puerto Rican prehistory (Periods III and IV, A.D. 350–1584), then a local style development corresponding to the evolution of the "Taino" culture which developed in Puerto Rico and spread westwards during those periods, should be discernible in the island's petroglyphic art. To demonstrate this through the advantages of more accurate recording is the main concern of this report.

It is generally agreed that the Island Arawak or "Igneri" originally came from South America, their language and basic cultural elements being Amazonian (Rouse 1948: 507). It is estimated that they took off from the east coast of Venezuela about the time of Christ and entered the West Indies by way of Trinidad and the Lesser Antilles, bringing with them pottery and agriculture but none of the ceremonial apparatus with which the Taino culture of Puerto Rico is identified. According to Rouse (1951: 261; Cruxent and Rouse 1958) the early arrivals (A.D. 1–350) settled only on the coast of Puerto Rico and did not penetrate the interior until Period IIIb (A.D. 750–1150) when a simple form of the "zemi" cult began to appear. The population of the interior only began to exceed the coastal population during Period IVa (A.D. 1150–1500) when ceremonialism and the zemi cult were reaching their peak.

According to Rouse (1948: 535):

> The *Taino* believed in the existence of spirits or souls, not only in their own bodies but also in some trees, rocks, and other natural phenomena. . . . The spirits of living people were called "goeiz"; those of the deceased "opia." Even during life the goeiz could leave the body; after death they went as opias to an earthly paradise called Coaibai, which was said to be a remote valley in some part of Hispaniola. . . .
>
> The *Taino* believed that by obtaining control over the spirits of nature and of their ancestors they would gain supernatural power. They did this by constructing idols of wood, stone, bone, shell, clay, cotton, or gold as places for the spirits to reside. These idols were called zemis; . . . [they] were also placed in various sacred

caves, some being carved on the walls in the form of petroglyphs. The rock carvings in the open in various parts of the island may also have been zemis.

He goes on to say that among the various powers of these zemis were the influencing of the weather, foretelling future events, regulating crops, improving hunting and fishing. In legends, rocks, rivers, and mountains were worshipped as ancestral points of origin.

To be worthwhile, any investigation of the Puerto Rican engravings requires not only a thorough exploration of the petroglyphic sites in all parts of the island, but also a careful detailed scrutiny and concise visual record of each site. With this object in mind, my husband and I spent 14 days in July, 1956, recording sections of the Cueva de los Indios (known also as Cueva Islote) 5 miles east of the town of Arecibo on the north coast. In 1957, over six weeks were spent in the central mountainous interior and the Luquillo region in the east. In view of what remains, our work may be said to have only just begun. Over 60 surface prints were made in the following areas (Fig. 1):

East

Municipality of Naguabo: three sites at various points along Río Icacos

Municipality of Río Grande: one site on Río Espíritu Santo

Municipality of San Lorenzo: one site on Río Grande de Loíza

Central Mountainous Interior

Municipality of Orocovis: one site on the eastern bank of Lago de Matrullas

Municipality of Jayuya: one site on Río Coabey, one site in Barrio Zamas, four sites at various intervals along Río Caonillas

Municipality of Utuado: one site on Río Grande de Arecibo (Barrio Salto Arriba)

North Coast

Several inscriptions on walls of Cueva de los Indios at Punta las Tunas, five miles east of Arecibo

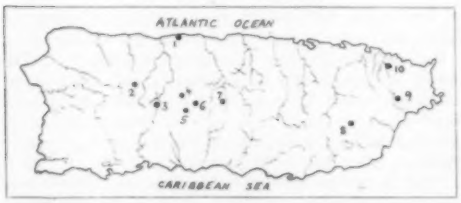

Fig. 1. Distribution of petroglyph sites in Puerto Rico. 1, Cueva de los Indios; 2, Capá; 3, Salto Arriba; 4, Caonillas; 5, Zamas; 6, Coabey; 7, Matrullas; 8, San Lorenzo; 9, Icacos; 10, Espíritu Santo.

As many more sites were explored and photographed and marked for future recording. To my knowledge, of the above sites listed, the rock shelters at Zamas and Icacos, and the sites along Río Caonillas have been hitherto unknown to archaeologists. For the purposes of this report, only Salto Arriba, Icacos, and Caonillas are discussed in any detail.

Recording Methods

For recording, we applied the "surface printing" technique we had experimented with in 1955 to reproduce the petroglyphs at Reef Bay, St. John, Virgin Islands. It consists of first preinking the *unengraved* areas of the rock by applying a water soluble printer's ink with foam rubber rollers and stencil brushes. A sheet of a non-woven cotton and rayon material known as Webril is then laid on. Dampened sponges applied to the surface of this material will cause the ink on the rock underneath to "bleed" through. A sort of monotype print is thus obtained. This is a variation of the technique for surface printing described by Mark Hedden (1958). Water base inks are easy to apply and wash off afterwards. Also the finished print dries quickly and can soon be rolled up with a precautionary sheet of newspaper between it and the next print. Later, the print can be ironed on to a fine linen (Chartex) which is impregnated with a petroleum and rubber mixture and acts as a waterproofing agent thereby preserving the ink and generally "fixing" the print. On the whole, although our method is slower and more laborious than that described by Hedden, it is suited to a larger variety of surface conditions, rough and bumpy ones in particular. In most cases a sharper image is obtained, including details which might otherwise come out garbled.

Puerto Rican petroglyphic sites are often off the beaten track, entailing long treks up and down rivers and mountains in a climate both hot and humid. At some rock shelter sites the engravings are found all over the surface including niches and recesses, along the sharp profiles of the rock, and on the ceiling. To make plaster molds and carry the necessary equipment for them would be cumbersome and difficult. The Latex mold method would be impossible at many sites due to dampness and encrustation and because its real effectiveness is confined to horizontal surfaces. Equipment for surface

printing on the other hand, is simple and easy to carry. The versatility of the material Webril helps greatly in overcoming distortions of the rock mass. In case of very large panels or unusually sharp angles, it can be cut and later fitted together like any pattern to give us a "map" of the engraved area. As illustrations in this article show, the degree of accuracy obtained over dubious methods of freehand sketching and chalking-in is considerable. The texture of the rock is preserved and above all, the spirit and esthetic quality of the aboriginal work of art is dramatically revealed.

In view of the apparent ease with which errors can be made regarding engraved designs, one cannot overemphasize the importance of long and careful study of the rock, involving observations under varying light conditions (especially important in photography), dousing with water, and searching the more elusive grooves with foam rubber rollers. Often several days must be spent on one site. In Puerto Rico it is foolhardy to approach an inscribed rock with the idea that what one sees upon first examination is all there is to it. The field worker using the surface printing technique will appreciate this more and more as he gains experience. Weathered grooves and reflected glare from a tropical sun do much to blind the eye. Certain mechanical aids such as polaroid dark glasses are a help. In photography, polaroid, and other light absorbing filters are necessary. Slow speed color film is surprisingly efficient in showing up the presence of designs on heavily eroded surfaces. Our project being essentially a visual one, photographic coverage is important. Ideally, the field worker should be equipped with a very sharp lensed reflex camera such as the Swedish Hasselblad which has interchangeable film magazines as well as lenses, enabling one to switch from color to black and white, or alternate between two different types of black and white film depending on what the situation requires. In addition, a Polaroid-Land camera is an asset not only as a social contact but also to take on the spot test shots of the site to be recorded. A photograph on hand of the pre-inked design is a very useful guide for the print maker, especially when dealing with large and complex engravings (Fig. 2).

One is reminded of the importance of accurate visual representation when referring to what are still our chief sources for illustrations of Puerto Rican petroglyphs (Pinart 1890; Fewkes 1903–04, Pls. 60, 61). Pinart did the most sketching, recording petroglyphs in other Greater and Lesser Antillean islands as well. Puerto Rico is represented by drawings of petroglyphs from Cueva de los Indios, Piedra Pintada at Cayey, Cueva de los Conejos near Arecibo, Cueva de Archilla near Ciales, Yauco, and Cano del Indio at Ceiba. His inaccuracy is criticised by Fewkes and indeed many of his recordings are difficult to identify with the originals. (It seems to be the custom of each new investigator to find fault with the accuracy of the former. Thus Koch-Grünberg [1907: 4] takes both Orsi de Mombello and Ernst to task; Fewkes [1903–04: 149, footnote a] dismisses Pinart who in turn scolds Ober [Pinart 1890]). A less prolific but more careful artist, Fewkes (1903–04, Pl. 60, Part 1) illustrates his version of some inscriptions found on a large boulder on the Río Grande de Arecibo in Barrio Salto Arriba, less than two miles south of the town of Utuado. These "Salto Arriba" rock carvings were presumably associated with a nearby dance plaza of which now little remains. In his drawing (reproduced here as Fig. 3), Fewkes suggests that Figure 3 *d, e* are "solar emblems" because of their "radiating lines." The same site was later visited by Rouse (1952: 495) who men-

FIG. 2. Method of surface printing a large complex petroglyph at Cueva de los Indios.

tions "human-like bodies and heads, of faces, and of geometric designs, several of which suggest the sun and the moon." Apparently for lack of a better visual representation of Puerto Rican petroglyphs, Fewkes's erroneous drawing appears again in an article on petroglyphs (Rouse 1949: 493–502). Strangely enough, compared to other petroglyphic sites in Puerto Rico, the Salto Arriba inscriptions are quite distinct. A photograph of them (Figs. 4, 5) shows quite clearly the nature of the so-called radiating lines of Fewkes's "solar emblems." Although such obvious solar symbols do exist elsewhere in Puerto Rico (Cueva de los Golondrinas, for example), in this case Figure 3 *d* could not be called such due to any emanating rays and belongs rather to the scalloped, or petaled, face type found in a number of other sites. Figure 3 *e* has turtle-like scales and would seem to represent this creature or some other scaly animal rather than the sun or the moon. The "horns" on the heads of Figure 3 *b, c* could just as well be the ears of a rabbit-like animal (this twin "rabbit" theme appears again at Espíritu Santo and rabbit-like heads are found among

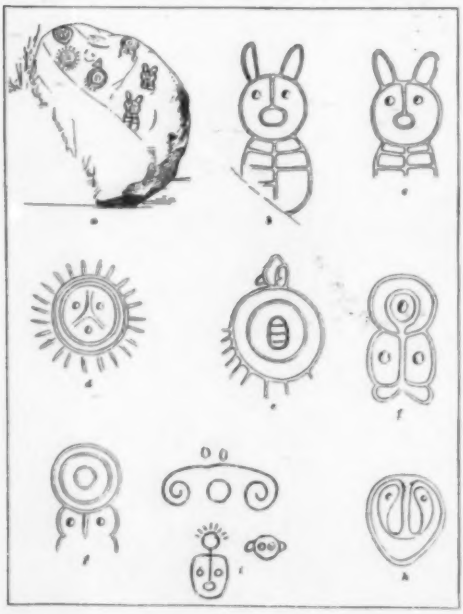

Fig. 3. Drawings of Puerto Rican petroglyphs made by Fewkes (1903–04, Pl. 60). All but *i* are from Salto Arriba. Compare Figures 4–6.

Fig. 4. Photograph of the boulder with petroglyphs at Salto Arriba. Compare Figures 3 *a*, 5.

the petroglyphs at Cueva de los Indios on the north coast and lower Río Icacos).

Fewkes overlooked a number of other engravings on the Salto Arriba boulder, including the two horseshoe-like symbols seen to the far left in Figure 4. Others are: a rain or water symbol (with vertical "tear" grooves) lying on the opposite shoulder of the rock at a point where a series of small rapids flow by, and the two heads facing each other (another recurrent theme) which lie on top of the rock (Fig. 6). Other omissions are to be seen in the surface print of the main Salto Arriba group (Fig. 5).

Techniques of Engraving

The Puerto Rican engravings were made on granite, granitic porphyry, quartz diorite, and limestone. At certain rocks, in the Río Icacos area and at Río Coabey, Jayuya, the Indians must have suspended themselves with lianas to make the carvings. In other cases, scaffolding or staging was probably erected. We ourselves had to do both to make recordings. A number of the river petroglyphs could only have been made during a very dry season. It was our good fortune to visit them during one of the severest droughts in the memories of the local inhabitants. River petroglyphs are found on boulders in association with rapids or waterfalls or the deep pools which form at the end of a series of rapids or waterfalls. They usually face directly across these phenomena or down river, or both. Occasionally, however, they are scattered all over the rock in every direction. We

FIG. 5. Surface print of the petroglyphs on the Salto Arriba boulder. Compare Figures 3, 4.

could not find any consistent east, west, north, or south directional tendency either on cave or rock shelter walls or on river boulders. Although some river rocks are "crowded" with carvings in varying states of preservation, we have not as yet found any superimposed. Grooves, some crudely, some finely made, were for the most part pecked out with a stone tool and smoothed afterwards by friction. In a number of sites grooves vary in character according to the specific intention of the engraver. Sometimes outcroppings of the rock which are suggestive are taken advantage of, giving an added sculpturesque effect. Negative incision seems to be rarer than in South America and the Lesser Antilles.

Pecked out groove depths vary from the 1¼-inch deep and equally wide grooves of some of the designs at the Cueva de los Indios, Arecibo (Fig. 2), to the less than 1/16-inch deep and ¼-to-½-inch wide groove of the petroglyphs at the Río Icacos rock shelter (Figs. 9, 10). The average, distinct, river petroglyphs are from ⅛ to ¼ inch deep and about ½ inch wide. However, at two river sites, Río Icacos and Río Caonillas, we find marked variations from the average depth and width of incisions. Allowing for uneven rate of erosion and occasional sloppy or unfinished execution, there are still the persistent traces of barely legible and presumably older engravings. They occur among, and some seem to be in the same style as, the more distinct designs whose grooves, as previously mentioned, average ⅛ to ¼ inch in depth. We began to pick up these faint traces only after several days of working at each site and becoming familiar with the rock surfaces under varying light conditions. Matters are further complicated by the presence of one or two exaggeratedly wide and deeper-than-average engravings of the *same style*. They stand out in the manner of a too-often pencilled-over drawing. The groove has widened to 1 to 1¼ inch and is ½ inch deep (Fig. 7, upper right). A possible explanation is that these designs were re-engraved either for amusement (Koch-Grünberg 1907: 78–9) or for a specific purpose. In South America, the Cubeo Indians, primarily a fishing people of the Uaupés-Caquetá region (Northwest Amazon), consider the petroglyphs at local rocky sites to be the creation of sib ancestors and

FIG. 6. Surface print of heads on top of the Salto Arriba boulder.

defend their river rights by citing the engravings as territorial validation. These petroglyphs are renewed from time to time (Goldman 1948: 784).

It is interesting to note that groove depths for the "deeply incised" type in South America are reported as follows: ⅛ to ½ inch (Mallery, after Brown, 1888–89: 144), ⅛ to ½ inch "or even more" (Im Thurn 1883: 394), more than ¼ inch "in protected places" (Farabee 1918: 168). In other words, they are the same, or if anything, deeper than the average groove depth of Puerto Rican petroglyphs. Re-engraving in South America up to and beyond historic contact could account for this and also provide an explanation for the inability of the Indians to explain the meaning of symbols of which they had merely become guardians.

Design Types

Puerto Rican petroglyphs comprise a variety of abstract and geometric forms. Among these are: concentric circles, spirals and double spirals (clockwise or counterclockwise, three to five ringed, plain or hooked, and, at Matrullas, with a snakes head in the center), volutes and double volutes, single and double hooks, two triangles set together resembling butterfly wings, with or without pits for mouth and eyes, the cross enclosed or slightly hooked at the four extremities suggesting a swastika form, horseshoe-like symbols, series of pits loosely grouped together.

Fig. 7. Petroglyphs at Caonillas showing possible re-engraving of the swaddled infant figure on the upper right.

Of a more descriptive nature are: multi-rayed solar emblems, simple frog, lizard, iguana, and bird designs, animistic heads with rays emanating from the neck, mask-like "faces," a variety of heads or faces, human and animal, ranging from simple circles with three pits or rings for eyes and mouth, to highly conventionalized headdressed types, some with emanating scallops resembling petals or feathers. Direct comparisons may be made with petroglyphs from Northwest Brazil (Koch-Grünberg 1907) and Venezuela (Tavera-Acosta 1956).

Swaddled Infant Type. In addition to the simple curvilinear and abstract forms, are two distinct "local" types or styles. The first, very frequent in the eastern and coastal areas, is a type described by Pinart (1890) as:

> . . . cette figure humaine paraissant emmailotee dans des langes comme un tout jeune enfant, la tête et le corps plus ou moins accompagnees d'ornaments et qui se rencontrent tres frequement . . .

The two figures discernible to the right in Figure 7 and those in the surface print from Upper Icacos (Fig. 8) illustrate this type, which occurs frequently in the eastern sector of Puerto Rico. The "Temehri" type from South America (Im Thurn 1883: 395) has, in Puerto Rico, changed into a chubby grinning or grimacing large eared primarily zoomorphic "swaddled infant," the limbless body rounded and sack-like at the bottom, the ears often exaggeratedly large and of varying shapes. Some have an amusing teddy-bear quality. Many have a variety of head ornaments. It is worthwhile comparing the general outline of these large-eared figures with certain problematical artifacts found near Fancy, St. Vincent, and reproduced by Fewkes (1912–13, Pls. 10–35, 38 *e*, *f*, 39, 52 *a*, 56, 58 *b*). The artifacts from St. Vincent are presumed to date back to pre-Carib times and are described as:

> . . . a series of fantastic objects made from metamorphosed volcanic scoria . . . several hundred specimens . . . found in a restricted area near Fancy at the base of the volcano La Soufriere. Nothing like them has been found in the adjacent islands, and it is quite probable that they were made and deposited at this place as votive offerings in way of propitiation to the god of the volcano.

On the whole, the infant type and the geometric and curvilinear types such as spirals, volutes, double hooks, tend to occupy separate sites. We are not yet sure whether this is a question of two different time periods (the

FIG. 8. Surface print of swaddled infant figures at Upper Icacos.

"spiral" groups are predominantly in the mountainous interior where the infant type is rarer), or is perhaps a difference in motivation and meaning. "Re-engraving" is evident in both.

Capá Type. Examples of the other separate petroglyphic style, not only in imagery but also in quality and depth of groove, are found at the Icacos, Caonillas, and Salto Arriba sites. Those situated on the northwest wall of the triangularly shaped rock shelter at Icacos (Fig. 9) are finely pecked and smoothed, less than $\frac{1}{16}$ inch deep and from ¼ to ½ inch wide. The design is discernible mainly because it is cut through a yellowish stain pervading the rock surface, and which is now starting to re-invade the groove at certain points (Fig. 10). On the opposite wall of the shelter, close to the entrance, is a solitary engraving of a realistically portrayed butterfly or insect, seemingly in the act of flying out of the shelter (Figs. 10, 11). The imagery and cutting technique here is different from the other two petroglyphic sites along Río Icacos which feature mostly the swaddled infant type.

At Caonillas, the correspondingly divergent style is in the form of a "dancing" figure with turban-like headdress, or diadem, swinging earspool, hands clasped as if in prayer, and a clumsy attempt at foreshortening (Fig. 12). Although not entirely successful at escaping the confines of conventional design, the intent is naturalistic, a portrait rather than a zemi. The figure faces downstream (east) and looks up at Cerro Morales (988 m.) towering in the distance. The texture of the groove, although in places eroded almost to the smoothness of the granite surface it is on, still shows the fine and even pecking technique found in similarly executed petroglyphs at the Icacos rock shelter and Salto Arriba (Fig. 6), and again on the pillar stones at the Capá, Utuado, dance plaza site (Figs. 13, 14). Its groove (⅛ inch deep) is somewhere between the average ¼ to ⅜ inch depth of the averagely distinct "swaddled infants" nearby, and the less than $\frac{1}{16}$ inch depth of barely legible traces of still the same infant type.

SEQUENCE OF TYPES

Unfortunately, available illustrative material of other West Indian petroglyphs is scant and represents only a few islands: St. Kitts (Fewkes 1903–04: 158, Fig. 24), St. Vincent and Grenada (Huckerby 1914, 1921), Curaçao, Aruba, and Bonaire (Pinart 1890; Hummelinck 1953, 1957), Guadaloupe (Koch-Grünberg 1907: 61; Mallery 1888–89: 140, Fig. 103), and the Bahamas (Mallery 1888–89: 138–9, Figs. 100–2). Krieger (1929, Pl. 5), Boyrie de Moya (1955), and Pinart (1890), have recorded pictographs and petroglyphs in Santo Domingo, and Herrera

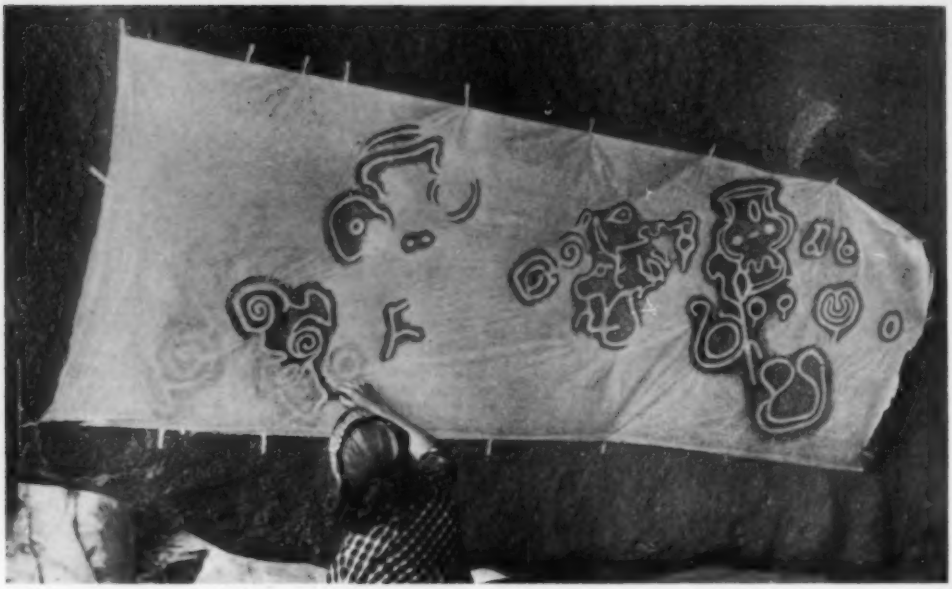

Fig. 9. Making a surface print of Capá type petroglyphs at the Icacos rock shelter.

Fritot (1938) the interesting pictographs at Punta del Este, Isla de Pinos, Cuba. In our own study of Puerto Rican petroglyphs we need an equally systematic, island by island, report from the rest of the Antillean area. At present, on the basis of available material, we have no way of knowing how far certain aspects of the infant type (already discernible in some Lesser Antillean petroglyphs) evolved before its final "Puerto Rican" stage. Tentatively, however, on the basis of Rouse's chronology and our own findings at Río Icacos, we believe that the swaddled infant type came into its own in Puerto Rico at least as early as Period III and represents the beginning of a "local" style, with basic design elements comparable to the South American shallow or "Temehri" type as defined by Im Thurn (1883: 391–7). As the latter diffused through the Lesser Antilles, its rather severe angularity underwent gradual modifications and eventually became the rounded and more expressive animistic type popular in Puerto Rico. The appearance of this petroglyphic type inland on the Río Grande de Arecibo and also its tributary, Río Caonillas, and its existence in pictographic (painted) form on the walls of Cueva del Templo, Samana, eastern Santo Domingo, indicates that the final realization of this style took place during Period III, since it was during that time that the major part of the interior was settled and that a population shift westwards into Santo Domingo occurred (Rouse 1952: 568–71). We would suggest that certain of the upper Río Icacos figures, those to the right in Figure 8, may be the earliest examples of this style because of

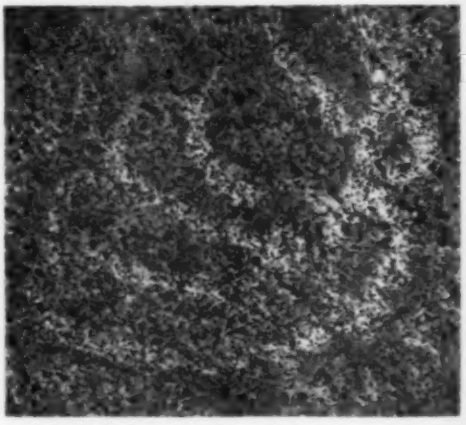

Fig. 10. Photographic detail of a finely pecked butterfly or insect at Icacos rock shelter. Compare the surface print in Figure 11.

their closer resemblance to petroglyphs from Guadaloupe (Koch-Grünberg 1907: 61).

The results of our excavation of a dwelling site located on a small plateau southwest of the hydroelectric station at a point where the Icacos joins Río Blanco, agree with the above hypothesis. The area is currently being used for tomato and vegetable farming and therefore much spoiled by continuous plowing. However, sufficient evidence was obtained from a relatively undisturbed 10-inch deep pit, with a heavy rate of sherd accumulation, to be able to classify it as early (unincised) Ostiones throughout, indicating Period III.

Finally, it is probable that the Capá type of petroglyphs with a shallower and, at first glance, seemingly older groove than their neighbors, are the most purely local and the most recent, whose final stage, characterized by the pillar stone carvings at Capá, Utuado, belongs in Rouse's Period IV (Figs. 13, 14). In this category are the two heads facing each other on top of the Salto Arriba boulder (Fig. 6), the dancing figure at Caonillas (Fig. 12), and the Icacos panel with naturalistic "butterfly" (Figs. 9–11).

Diagnostic of the style is the shallower, more evenly controlled groove (no re-engraving), a tendency towards naturalism, almond shaped "negative" eye pits more deeply incised than the rest of the design, special treatment of the nose and lips and eyebrows, similar to the stone masks and sculptured heads of higher Tainan art, and the appearance of the earspool (a ceremonial trait) in the form of a punctured disc. The circle with a dot in the middle becomes a dominant motif in later Tainan pottery decoration, figurines, three pointed stones, and ceremonial duhos. Further indications of this being the latest petroglyphic style in Puerto Rico, is the condition of the groove itself. In reconstructing the dance plaza site at Capá, Mason (1941: 223) overturned a number of pillar stones which had fallen on their faces and been buried. One of these shows an unweathered groove (Mason 1941, Pl. 7, Fig. 2). We also re-erected a fallen pillar stone at Capá in 1957 and found a design whose groove was lighter than the rock, and appears freshly made (Fig. 14). Since we know that weathering is not a very long process, we can deduce that the courts started to fall into disrepair, or were deliberately despoiled, soon

FIG. 11. Surface print of the butterfly or insect at Icacos shown in Figure 10.

FIG. 12. Surface print of a dancing figure at Caonillas.

Fig. 13. Petroglyph on pillar at dance plaza site of Capá, Utuado.

Fig. 14. Petroglyph on pillar at dance plaza site, Capá, Utuado, showing fresh and unweathered state of the grooves.

after these carvings were made. Note that the protected groove inside the Icacos rock shelter still has this fresh aspect (Fig. 10).

At Icacos and Caonillas, supporting archaeological evidence for corroborating our theory of the recentness of these petroglyphs is unfortunately meager. We know only from Fewkes (1903–04: 82) of a dance plaza or ball court being reported from Barrio Paso Palma in which the Caonillas sites are located. Apart from two petaloid celts in our possession and reportedly found near the Icacos rock shelter site, we know only of a "very fine stone mask" which De Hostos reports was found in the vicinity.

Future Study

This report is necessarily incomplete in lieu of a comprehensive coverage of the entire island of Puerto Rico. We especially need to study the western sector which we have not yet visited and where the Indians are said to have survived longest.

In the future, we plan to treat certain sites which seem to comprise a single stylistic unit, such as the Zamas rock shelter and the Matrullas area, as separate studies. Also it will be important to make a more systematic study of recurrent symbols and eventually include a comparative study with other Caribbean areas. Since we have demonstrated the importance of more accurate recordings, this will probably involve similar careful and detailed investigations in the rest of the Antilles and eventually also those South American areas directly concerned.

Finally, if we are to stay within Rouse's time period for Puerto Rican petroglyphs, we are faced with the problem of those faint traces of what appear to be earlier engravings at Caonillas, Icacos, and other river sites. Some of these are identifiable as being of the swaddled infant type (Icacos and Caonillas). We find the groove of these eroded symbols appreciably narrower than their more distinct counterparts. Was re-engraving, then, a general practice with occasional emphasis on certain symbols, the latter being perhaps clan totems? In order to answer this question it will be necessary to devote more time and special efforts in revealing these faint engravings in order to establish their nature more clearly, although it is unlikely that we shall ever be able to take completely accurate prints of them.

Acknowledgments. I am deeply indebted to Irving Rouse for his cooperation and generosity at all times during this study including the use of his file of petroglyphic sites in Puerto Rico; to Ricardo Alegría, Instituto de Cultura Puertorriqueña, San Juan, for additional help and information concerning sites; to artist Sari Dienes for information regarding use of the Kendall Mills non-woven fabric, Webril; to Don Bernardo de Paz and family, Alcalde Adrian Torre, Manuel Gonzáles, and Efraín Reyes Gonzáles at Jayuya, and to hydroelectric plant director Alberto Riefkohl, Felipe and Damas and their families at Río Icacos, and the hospitable jíbaros of Puerto Rico without whose help we could never have managed; and to my husband, Roberto, official photographer and enthusiastic collaborator.

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MODERN CUICATEC USE OF PREHISTORIC SACRIFICIAL KNIVES

WILLIAM R. HOLLAND AND ROBERT J. WEITLANER

ABSTRACT

Three Mixtec human sacrificial knives of the late Postclassic period were found in 1957 at the Cuicatec village of San Andrés Pápalo in northern Oaxaca, Mexico. They were being used to sacrifice chickens and turkeys as offerings in curing ceremonies at a ruined prehistoric site on top of the mountain which rises above the village. Offerings of food and drink are made to the spirit of thunder who lives on the mountain top and who has power to control longevity and cure the sick. The three knives and a conch trumpet were stored in a ceremonial cave near the village. One wooden handle is plain; the others are carved to represent a human hand and the head of a serpent. The handles lack the elaborate mosaic decoration which distinguishes the few prehistoric sacrificial knives in museum collections. These three knives constitute material evidence for the survival of a prehistoric ritual system which involved the making of blood sacrifice to rain gods on mountain tops. The shift from human to animal sacrificial victims is probably the major change in the prehistoric pattern.

THERE ARE few places in contemporary Mexico that do not have at least some archaeological and ethnological survivals of prehistoric culture. One such tradition which has endured into modern times is that of blood sacrifice. Animal and human sacrifices were probably quite common in Mesoamerica for centuries before the coming of the Spaniards. Although human sacrifice ceased with the conquest, animal sacrifice has survived to the present day in the areas of greatest Indian concentration, in central and especially south Mexico.

A survival of sacrificial ceremonies in which prehistoric sacrificial knives are used was recorded at the Cuicatec Indian village of San Andrés Pápalo, Oaxaca, Mexico (Fig. 1). These knives were discovered in March of 1957 while we were conducting an ethnographic, linguistic, and archaeological survey of the Cuicatec and Chinantec regions for the Mexican National Institute of Anthropology and History. The expedition, under Weitlaner's direction, arrived at Cuicatlán, Oaxaca, on February 16, 1957, and left within a few days by horseback for the Cuicatec villages high in the surrounding mountains to the east. After spending two weeks visiting the first four villages, Concepción Pápalo, Santos Reyes Pápalo, Santa María Pápalo, and San Pedro Cuyaltepec, the expedition arrived at San Andrés Pápalo, where the knives were discovered.

THE VILLAGE OF SAN ANDRÉS PÁPALO

In many ways San Andrés Pápalo is typical of Indian settlements in south Mexico. Its population probably does not exceed 300, and there are no stores or markets. Manufactured goods must be brought from Cuicatlán, two days distant by horseback. The usual house construction technique employs pole walls and thatched roof. Practically all men are farmers and there is very little specialization of labor. Some women still weave cloth and make pottery, but they are not often seen dressed in the traditional *huipil*. Most of the men and older boys speak Spanish as well as their native language. The culture of this village then, is essentially Indian with the addition of many elements of modern Mexican mestizo culture.

San Andrés Pápalo is situated at the foot of the western slope of the high and imposing mountain and from time to time registers his the place from which thunder originates. The natives believe that the *Sa?iko* inhabits the mountain and from time to time registers his displeasure with them by causing thunder to emanate from the top of "Thunder Mountain." The mountain spirit is often confused with the Catholic saint, San Jerónimo, who is believed to inhabit the mountain also and to possess powers similar to those of the mountain spirit.

The mountain spirit plays a very important role in the lives of the villagers because they believe he is able to control longevity and return the ill to good health. The village curers supplicate the mountain spirit for help in alleviating the suffering of an afflicted person by journeying to the top of the mountain and making offerings in a ruined prehistoric site. In addition to many items of food and drink, these offerings usually include the sacrifice of chickens or turkeys carried out on the mountain top with the prehistoric sacrificial knives described here (Fig. 2).

THE SACRIFICIAL KNIVES

We sent two Indians who had been in our employ during our stay in the village to investi-

gate a nearby cave reportedly used for curing ceremonies and witchcraft. The natives of the village did not want members of the expedition to visit the cave in person. According to the account of the two Indians, they found the three knives and a conch shell trumpet (Fig. 3) leaning against the wall when they entered the cave. The cave was quite dry. The excellent state of preservation of the wooden handles of the knives suggests that they had been kept in essentially dry surroundings since their manufacture. The knives were purchased for a nominal sum and brought to Mexico City where they were presented to the National Museum of Mexico. They were later examined and identified by Alfonso Caso as Mixtec in origin, and were tentatively assigned to the late Postclassic period immediately prior to the conquest.

All three knives have flint blades hafted with copal in wooden handles carved from *Juniperus sabina*. The largest knife measures 35 cm. long (Fig. 2 *a*). It has an undecorated handle 20 cm. long and 3 cm. wide. The blade is 21 cm. long and 7 cm. wide. The handle overlaps 6 cm. of the blade. The knife with the handle carved in the form of a human hand is 28 cm. long (Fig.

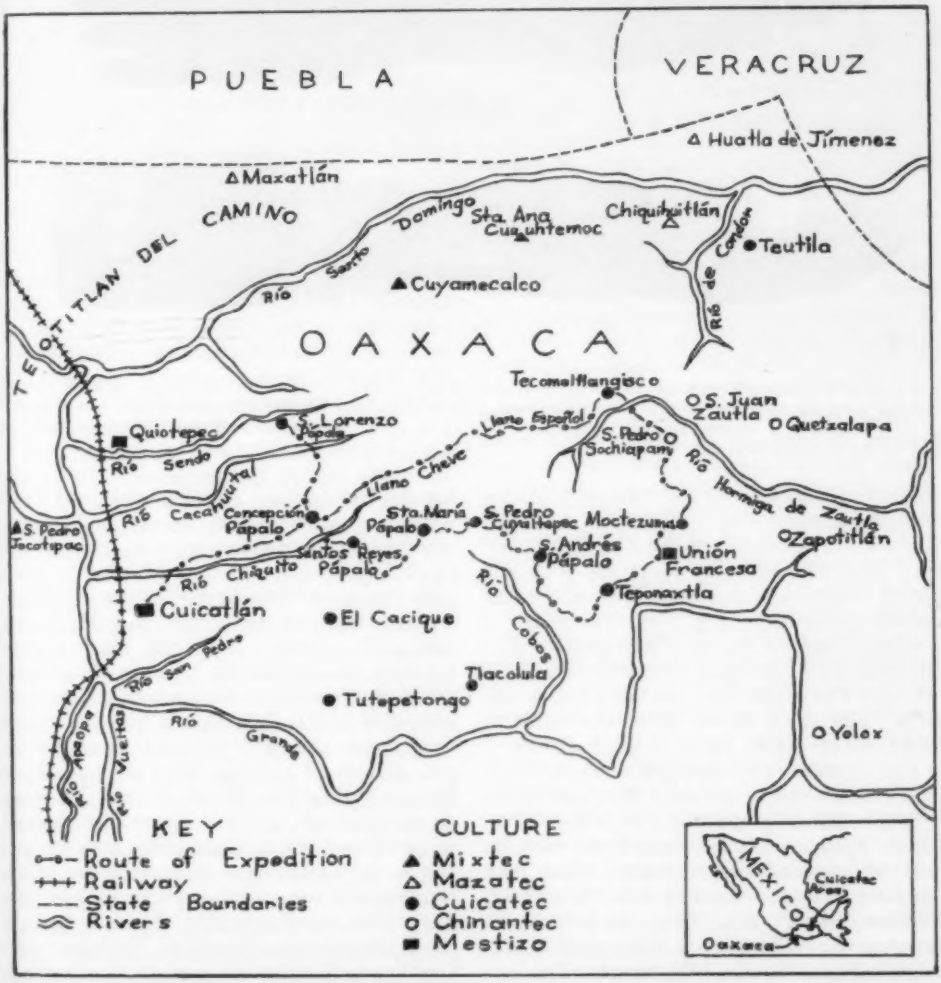

FIG. 1. Map of part of northern Oaxaca showing the route of the expedition. Drawn by Elizabeth Holland.

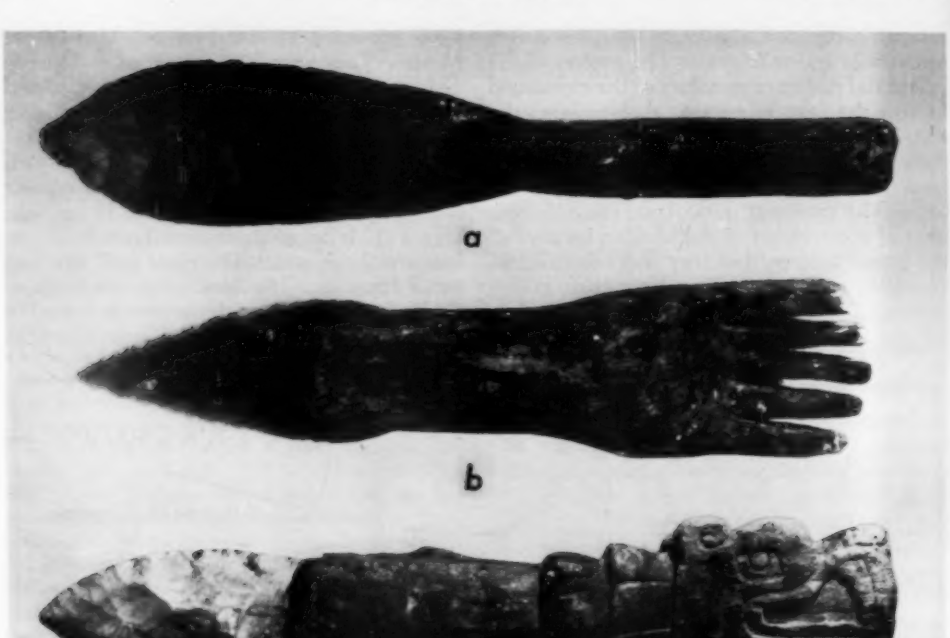

Fig. 2. Prehistoric Mixtec sacrificial knives from San Andrés Pápalo, Oaxaca. *a*, undecorated wooden handle; *b*, wooden handle carved in form of human-like hand with palm and five fingers; *c*, wooden handle carved to represent the head of a serpent, possibly Quetzalcoatl. Length of *a*, 35 cm. Photos courtesy of V. Ojeda and the staff of the National Museum of Mexico.

2 *b*). The handle is 19 cm. long and 5.5 cm. wide, the blade, 13 cm. long and 5.3 cm. wide. The amount of overlap of handle on blade is 6 cm. The most elaborate knife has a handle carved to represent the head of a serpent and possibly Quetzalcoatl (Fig. 2 *c*). The length of the entire knife is 30 cm. The handle is 20.5 cm. long and 5.5 cm. wide. It overlaps the blade 3.5 cm. The blade is 12 cm. long and 4 cm. wide. The three blades differ somewhat in shape, but all exhibit careful pressure flaking.

The number of prehistoric Mesoamerican sacrificial knives that have found their way to the various museums of the world is not large. The British Museum has an Aztec knife with an elaborate mosaic-encrusted handle which may represent an eagle warrior (Saville 1922, Pl. 38; Vaillant 1941, Pl. 60). There are two similar knives in the Prehistoric and Ethnographic Museum in Rome (Saville 1922, Pl. 39). The mosaic-encrusted handles are in the form of crouching human figures. Another elaborate knife with a mosaic and gold decorated handle representing two intertwined serpents, now in the Peabody Museum, Harvard University, came from the Sacred Cenote at Chichén Itzá (Morley and Brainerd 1956: 209, Fig. 14; Tozzer 1957: 172). Tozzer (1957: 198) also mentions a carved wooden handle for a sacrificial knife which also came from the Sacred Cenote. Proskouriakoff (1952: 259) reports that "two sacrificial knives like those recovered from the Sacred Cenote at Chichen Itza" were found at Mayapán. The San Andrés Pápalo specimens do not share any specific features with the mosaic-encrusted knives. Comparison with the second Sacred Cenote example and the two Mayapán knives is not possible without an examination of the actual specimens because they are not illustrated in the literature. The three sacrificial knives found in use at the Cuicatec village of San Andrés Pápalo are the only authentic

FIG. 3. Conch shell trumpet found with the three sacrificial knives.

prehistoric examples of this important ceremonial artifact on display at the National Museum of Mexico.

THE CUICATEC SACRIFICE ON THUNDER MOUNTAIN

In times of crisis such as an acute illness or recent birth in the family, anxiety about survival and restoration to good health is especially great. As there are no modern medical facilities for the Cuicatec to draw upon in such cases, they depend upon the traditional folk medicine concepts and practices. When the case is severe, the usual practice is to engage one of the village's part-time curers. Practically any respected adult in the village who claims special knowledge of these problems can fill this role. The curer comes to the ailing person's hut and begins to diagnose the malady. If it is decided that the intercession of the mountain spirit would be desirable, the curer leaves the hut and prepares to ascend "Thunder Mountain" to make an offering and sacrifice to the *Sa?iko*.

Before beginning the ascent, the curer goes to the cave where the sacrificial knives are kept. He selects one of the three knives and then returns to the village where he collects the items of the offering from the family of the patient. The offering usually includes such things as *carrizo* (hollow sticks) filled with *tepache* (an alcoholic beverage derived from fermented sugar cane pulp) with leaves wrapped around them, *zempaxochitl* flowers, eggs, pieces of *petate* mats, bananas, oranges, and various other fruits. The offering may include any or all of these items, selection depending on the nature of the illness, the preferences of the curer and the resources of the family. The offering may also include the sacrifice of a chicken or turkey. When the appropriate items are collected the curer makes the long, steep ascent of the narrow path to the top of "Thunder Mountain." The journey is made alone and in the middle of the night.

On the top of the mountain there is an archaeological site which consists of remnants of a long wall, a floor paved with stone blocks, and a stairway. The wall is approximately 30 m. in length and 2 to 3 m. in height. The remains of the floor are not much in evidence and are nearly covered with earth and vegetation. On the southern extremity of the site there is a deteriorated stairway of four steps which is approximately 3 m. wide and 1 m. high. The construction material consists of roughly cut rectangular slabs measuring approximately 1 m. long, 33 cm. wide, and 20 cm. thick. No mortar was apparent in the construction.

The curer reaches the archaeological site and then follows the path to a place on the exact top of the mountain where the offering hole is located. When he arrives at the sacrificial site the hole is covered by small rocks and so he must remove them in order to prepare to make the offering. Once ready, he raises the turkey toward the heavens with outstretched arms. If it is the case of a newborn child, he supplicates the *Sa?iko* with these words in Cuicatec: "As this turkey lives, so may live the child." He then decapitates the fowl with the sacrificial knife. Later he cuts off the legs and deposits them and the head in the sacrificial hole, retaining the edible parts for himself. He then deposits the other items of the offering, if any, in the hole. If the case is one of illness, the curer directs himself to the mountain spirit in these terms as he sacrifices the fowl: "As this turkey is healthy, so may ——— be healthy again."

The turkey or chicken sacrifice is the principal part of the offering and after it is finished each item is deposited in the same way as above. When the ceremony is completed the curer returns the stones to their places descends the mountain and returns to the village.

Animal sacrifice to mountain spirits is quite common and widespread in this area. Similar practices are found among the nearby groups such as the Mazatec (Villa Rojas 1955: 119), the Zapotec (de la Fuente 1949: 307), the Mixe (Beals 1945: 85), and others. A more extensive survey of the literature would undoubtedly reveal that this trait is common in the areas of Mesoamerica where Indian culture still prevails.

Sacrifices in Prehistoric Times

Blood sacrifices on mountain tops were probably commonplace in Mesoamerica in prehistoric times. In prehispanic Mexico the custom involved humans as well as animals. Dahlgren's (1954: 270–1) description of the ancient rite among the Mixtec offers a typical example of human sacrifice in those times:

> The fact that the peaks of mountains were sacred places is confirmed by both archaeology and the prehispanic chronicles. The mountains of the Mixtec area are literally sown with ruins. The Mixtec had their idols in the highest places and they sacrificed humans there. In Yanhuitlán when a sacrifice was made to the water god the priest carried the idol to the highest place and did the sacrifice there.

The association of mountain ceremonies to rain gods and concepts of health and general well-being among the Aztecs is brought out in this passage from Sahagún (Bandelier 1932: 45):

> All prominent mountain peaks, especially such around which rain clouds will gather, they imagined to be gods, therefore they made of each an image according to their idea. They also thought that certain diseases which are due to the cold or inclement weather came from the mountains, and that these mountains had the power to cure them. Therefore, all those who became ill of such diseases made a vow to offer a feast and offering to such and such a mountain closest to which they happened to live, or to which they were most devoted.

Summary and Conclusions

It is probable that the animal sacrifices to mountain spirits that take place in San Andrés Pápalo and other contemporary Mexican Indian villages are survivals of prehispanic beliefs in rain gods. These sacrifices are still made today as they were in the past in order to supplicate the mountain spirit for help in curing the ill and inducing longevity. The location of the present day sacrifices on the tops of mountains appears to be unchanged since preconquest times.

The most important material survivals of this practice discovered at San Andrés Pápalo were three late Postclassic Mixtec sacrificial knives and a conch shell trumpet. Probably the most significant alteration in the practice of blood sacrifice since prehispanic times is the shift from human to animal sacrificial victims.

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May, 1959

Sylvanus Griswold Morley and Juan Martínez Hernández. (Photo by Giles G. Healey.)

JUAN MARTÍNEZ HERNÁNDEZ

1866–1959

On the afternoon of March 15, 1959, I drove to Itzimná, on the outskirts of Mérida, Yucatán, the last of a long line of gringo students of Maya culture to make their way there to visit the Maya scholar, Juan Martínez Hernández. His home, simple in its modest setting of untended *quinta*, is not yet engulfed by growing Mérida. Patient *Zopilotes*, the rustle of wind-torn banana plants, and a pleasant air of "untidied-upness" contrast with the hurry and cacaphony of the center of Mérida or the opulent oleander and royal palms of the wealthier suburbs.

The mourners, for Don Juan had died the previous day, almost all in the Yucatecan dress of white trousers and white coat or *guayabera*, chatted in small groups on the veranda and in the *quinta*; in the *Capilla ardiente*, within, candles cast strange shadows on the tall crucifix and the tightly-shrouded figure before it. The life, just brought to a close, had spanned nearly a century. When Don Juan was born, Maximilian was still Emperor of Mexico; his early twenties were the teething years of Maya archaeology: Charnay was exploring Yucatán in search of Tula influences, Maudslay was at Chichén, and Eduardo Thompson was at Labná on Harvard's first expedition to Middle America.

Juan Martínez was born in Mérida, December 31, 1866. He studied at San Ildefonso College, Mérida, and at Georgetown University. Much of his time was spent at his hacienda of San José Boxactún, close to the port of Telchac, and where Maya was surely more often on his lips than Spanish.

His Maya interests were first roused by his enjoyment of Stephens' *Incidents of Travel in Yucatan*, and he was drawn at an early age to the study of the written Maya sources, where his fluency in modern Maya was, naturally, immensely useful. Ralph Roys has emphasized to me that the Martínez translation of the Chumayel

(Continued on next page)

creation story (published in 1913) before photos of the Motul dictionary were available was a remarkable achievement. Roys relates that when he first met Martínez, in 1921, he asked him how he had done it. Martínez spent the afternoon demonstrating how he broke polysyllabic words down into their parts, and, getting every meaning of each, selected those which best fitted the context, and rebuilt the whole word. Roys also spoke of the generous help Martínez gave him when he was preparing his translations. Over the years aid and comfort were freely given to many.

From 1913 to 1915 Martínez was inspector of archaeological monuments in Yucatán, but he resigned that position in favor of his son, Eduardo, on being appointed chief of the Department of Industry and Commerce by the Governor of Yucatán, General Salvador Alvarado. Martínez was deeply interested in the henequen industry. He had planted his hacienda with henequen and, as early as 1907, he founded *El Agricultor*, journal of the Cámara Agrícola de Yucatán. Finally, he was sent to New York as representative of the very important henequen price control commission. Ill health caused him to resign this important post.

The year 1926 witnessed publication of a spate of extremely important translations from the Maya and his *Paralelismo o entre los calendarios maya y azteca*. This revived the Goodman correlation of the Maya and Christian calendars, and was a turning point in Maya research. The search for astronomical proof of his arrangement was the subject of many of his later papers. In 1929, with the pecuniary aid of T. A. Willard, he published the Maya-Spanish part of the famed Motul dictionary and the extremely rare Coronel *Arte*. There was criticism of this, but typesetting was not all it might have been in the Yucatán of 30 years ago, and as quickly as Don Juan corrected one printer's error, another appeared in the resetting. Luckily few of these are of consequence, and the value of a printed edition far outweighs the importance of the errata.

Maya blood ran in the veins of this brilliant pioneer in the field of Maya research. I am deeply grateful to Margaret Currier, Antonio Canto López, Eduardo Martínez Cantón, and Ralph Roys for help in preparing this inadequate tribute to a man of high culture.

J. Eric S. Thompson

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[These three items apparently were translated into Spanish by Martínez from transcripts by Adela C. Breton of the Maya originals in the Peabody Museum, Harvard University. The Spanish versions were then translated into English by Miss Breton. The three are together in the Peabody Museum Library, call number C.A. 6 B756m.]

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AMERICAN MUSEUM OF NATURAL HISTORY FELLOWSHIPS IN ANTHROPOLOGY

Two resident research fellowships, to be known as the *Ogden Mills Fellowships*, are to be awarded annually by the Department of Anthropology of the American Museum of Natural History, New York City. The fellowships are intended for anthropologists who might benefit by a year at the Museum, using the anthropological collections, the library, and other facilities of the Museum, or by working in conjunction with members of the curatorial staff. There is no specific limitation on the kind of anthropological research that may be done under these fellowships, but preference will be given to those applicants who can make use of the Museum's collections and archives (such as unpublished field notes, films, sound recordings) either in the elaboration of special studies or in connection with research they, or members of the curatorial staff, may have under way. It is not the intention to support field work by means of these fellowships, although it is possible that such activities may occasionally be included.

Candidates within their early post-doctoral years will be given preference, but otherwise there are no rigid requirements in terms of age, sex, degrees, or academic background. Applicants will be judged on the basis of their records and on the worthiness of their research aims. Selections will be made by the staff of the Department of Anthropology, and the right is reserved to withhold the award of fellowships in any given year. The period of the fellowships is for one year, beginning June 15; the stipend for each, $5,000. First appointments will be made for the year 1960–61. Applications should include a full curriculum vitae, a letter describing the candidate's research aims, and supporting references, and should be addressed to Harry L. Shapiro, Department of Anthropology, American Museum of Natural History, New York 24, N.Y. It is hoped that the 1960–61 fellows can be selected from applications received before March 1, 1960. Dates for receiving applications and making decisions on appointments will be adjusted in following years.

FACTS AND COMMENTS

NELS C. NELSON AND CHRONOLOGICAL ARCHAEOLOGY

RICHARD B. WOODBURY

ABSTRACT

A series of papers at the 24th Annual Meeting of the Society for American Archaeology, in Salt Lake City in 1959, was devoted to Developments in Dating Techniques, and dedicated to Nels C. Nelson in recognition of his pioneering role in establishing the technique of stratigraphic excavation in America. During his work in the Galisteo Basin of New Mexico in 1914 he dug a series of ten 1-foot levels, classified all the sherds found in them into seven types, and calculated their frequencies by levels. These resembled sections of normal distribution curves, and demonstrated that statistical analysis of data from arbitrary levels could reveal chronological change just as could data from physically distinct strata. Within a few years this technique was employed by several other archaeologists and it continues to the present to be an indispensable technique of investigation.

AS ARCHAEOLOGISTS we sometimes seriously neglect the past — the past, that is, of our own discipline. We often take for granted the ideas, the techniques, and the conceptual tools which make our work possible. The series of papers on Developments in Dating Techniques which was presented on April 30, 1959, at the 24th Annual Meeting of the Society for American Archaeology in Salt Lake City seemed an appropriate occasion on which to recall the relative recency of American archaeologists' effective concern with chronological studies. There must immediately come to mind, in this connection, the man who is chiefly responsible for the introduction to this country of one of our fundamental chronological techniques. It was for this reason that the session mentioned above (which was jointly sponsored by the Society for American Archaeology and the Tree-Ring Society) was dedicated to Nels C. Nelson.

In 1913, at the American Anthropological Association's annual meeting in New York City, the Presidential Address was devoted to "Some Aspects of North American Archeology," and the speaker discussed at some length the "relatively . . . simple and intelligible" state of the archaeological record for western America in contrast to the "complex and . . . baffling" situation in the east. He described the lack of any evidence for cultural changes along the entire Pacific Coast and concluded that there was "no evidence for any succession of distinct cultures" in that area. For the east he found a far more varied but equally timeless picture and suggested the need for systematic attention to the relation of historic to prehistoric occupations as a start toward understanding the complexity of cultural assemblages and their possible sequence. It would be interesting to know precisely what principles Dixon (1913: 566) had in mind in his closing query:

> Is it not something of a reproach to American Archeology that it has so far failed to realize and appreciate, as fully as it ought, the need of applying to the solution of its problems the principles which have, in other lands, led to such substantial and magnificent results?

However, the situation was less shameful than Dixon realized. Only a few months before, also in New York, at the March meeting of the American Ethnological Society, Nels C. Nelson had spoken on "The Galisteo Pueblos," giving a first account of archaeological reconnaissance and excavation of the previous summer. A monographic report of this work was published by the American Museum of Natural History in 1914 (Nelson 1914), and in 1916 (Nelson 1916) there appeared in the *American Anthropologist* a detailed report of a phase of the work through which Nelson was already profoundly influencing many of the ablest of America's younger archaeologists.

Prior to his work in New Mexico, Nelson had excavated the Ellis Landing Shellmound (Nelson 1910) on the shore of San Francisco Bay; in this first field work, begun in 1906, Nelson had been careful to cut a straight, clean face at one side of his main trench through the mound, and to chart all its details as the work progressed, as well as to record the location of all artifacts in relation to it. This was not a unique or original accomplishment, but published accounts of other excavations in America at that period indicate that Nelson's technique represents a rarely practiced refinement which provided for a precision of observation and recording far beyond the customary. Superposition and depositional sequences were all too often observed only impressionistically and recorded (if at all) as vague general conclusions without supporting data. It should be noted that Nelson's published report shows resemblances in both excavation and observational techniques to Max Uhle's report on his excavation of the Emeryville Shellmound in 1902 (Uhle 1907). Uhle was an experienced archaeologist, trained in Germany; in his brief excavations in California, as has been pointed out by Clement W. Meighan, he "recognized, in the small sample of materials secured, elements of stratigraphic difference which were later confirmed by more extensive work" (Rowe 1954: 8). Nelson was undoubtedly influenced by Uhle's work, but both of them found the paucity of artifacts in the San Francisco Bay shellmounds a difficult basis for demonstrating cultural change from level to level. It is also probable that Nelson was considerably influenced by John C. Merriam, the noted palaeontologist, who conceived and directed the program of investigating the San Francisco Bay shellmounds. Merriam took part personally in some of the work, and since he was a meticulous investigator undoubtedly insisted on careful observation and recording by his student assistant.

When Nelson investigated the Galisteo Basin in the summer of 1912, one of his aims was the establishment of a chronology for the several types of ruins and numerous styles of pottery already familiar to those studying the ruins of the Rio Grande region; he says, with gener-

ous understatement, of the several years' work by the Archaeological Institute of America in the Pajarito Plateau area, that "the conditions here do not seem thus far to have yielded precise chronological information" (Nelson 1916: 161). Besides clearing many rooms in the Galisteo ruins and excavating numerous burials, Nelson looked for accumulations of refuse that would offer clear evidence of cultural change and the succession of occupations. None of the refuse mounds that were trenched in 1912 and 1913 fully satisfied him, because the repeated digging of prehistoric graves into the trash had disturbed the accumulations too greatly. Late in the season of 1914 Nelson found a satisfactory deposit at Pueblo San Cristóbal which showed no signs of disturbance in the 10-foot face that was cleared. He marked out a block 3 by 6 feet and dug it out in 1-foot levels, saving all the sherds encountered. Nelson comments that he did all the digging himself, not trusting to the care of his workmen. The sherds were classified into seven "types," each "sufficiently distinct" although admittedly arbitrary in definition; they were then counted and their numbers plotted by levels (Nelson 1916: 166). Nelson points out that each type shows a portion of a normal distribution curve, and demonstrates that "the style of pottery indicated came slowly into vogue, attained a maximum and began a gradual decline" (Nelson 1916: 167).

Familiar as such a procedure is today, in 1914 it was a brilliant innovation, and it was immediately recognized as a powerful tool for archaeological research. As Spier (1931: 275) has pointed out, Nelson's paper in the *American Anthropologist* was "the first exposition of a refined method for determining exactly the time sequence of archaeological materials in a primitive area." It was certainly high time that American archaeologists began establishing exact local sequences, such as had already become familiar in geology. Nelson provided the means for this, using the long-familiar geological principle of superposition, in order to relate successive deposits in a stratigraphic sequence. He also added the refinement of excavating by arbitrary and uniform levels, rather than depending on visibly separable strata, as had been done for some years in Europe and was characteristic of geological studies of stratigraphy. Nelson's technique was, in fact, a significant modification of and supplement to the European archaeological approach, which many Europeans were slow to appreciate. No less an expert in field methods than Sir Mortimer Wheeler still refers (1954: 53) to it as "the old outworn system, with its mechanical 'unit-levels'" and mentions Pengelly's use of the technique in Kent's Cavern in 1865 as though it were quaint but unfortunate. (I am indebted to John H. Rowe for reminding me of Wheeler's comments.)

A. V. Kidder, who had visited Nelson in the field during his Galisteo work, began stratigraphic investigations during the first season at Pecos in 1915. A. L. Kroeber and Leslie Spier made stratigraphic tests in the pueblo of Zuni and in ruins nearby in 1916. More or less concurrently with Nelson's work the International School of American Archaeology and Ethnology in Mexico undertook stratigraphic excavation in the Valley of Mexico at the instigation of Eduard Seler and under the immediate direction of Manuel Gamio, A. M. Tozzer, and Clarence Hay. Examples could be multiplied of the rapid and successful adoption in the New World of the stratigraphic technique. For a century and a quarter stratigraphic relationships had been correctly interpreted by occasional archaeologists in America (see Heizer 1959: 215), but not until Nelson's demonstration of it again, in the Galisteo ruins, did it become a recognized and indispensable approach.

The further refinements of stratigraphic work have been many, but the principles enunciated so clearly by Nelson remain unchanged. Also, many new chronological techniques have been devised, providing absolute chronology for sequences that were previously only relative. But it will be generally agreed that refined stratigraphic excavation, with comparisons of levels in terms of artifact-type frequencies, continues to be one of the surest and most-used tools of the archaeologist. It is a basic technique that we could not work without.

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ARCHAEOLOGY AND THE PUBLIC IMAGE

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Abstract

The archaeological content of ten years of *Life* magazine is analyzed in an attempt to identify what may go into formulating the public's images of the archaeologist and his goals. The four themes which appear in the 34 *Life* articles are: chance nature of archaeological discovery, role of the archaeologist as an expert, emphasis on technical knowledge and skills, and heavy use of superlatives. Analysis of other mass media, including fiction and cartoons, might lead to the identification of other themes. The image of archaeology presented by mass communication is considered important in a science so dependent upon public cooperation.

A. V. KIDDER identifies two popular images of the archaeologist in his 1949 introduction to Amdsden's *Prehistoric Southwesterners from Basketmaker to Pueblo*: the hairy-chinned and the hairy-chested. Kidder's description of each type is as follows:

> Happily, the hairy-chested variety rarely occurs save in the rotogravure sections of the Sunday papers and in the advertisements of a certain well-known brand of whiskey. There one sees him as a strong-jawed young man in a tropical helmet, pistol on hip, hacking his way through the jungle in search of lost cities and buried treasure. His boots, always highly polished, reach to his knees, presumably for protection against black mambas and other sorts of deadly serpents. The only concession he makes to the difficulties and dangers of his calling is to have his shirt enough unbuttoned to reveal the manliness of his bosom.
>
> The hairy-chinned archaeologist exists, for the greater part, in the *Saturday Evening Post* and *Colliers*, usually as the father of a beautiful girl in jodhpurs. He is old. He is benevolently absentminded. His only weapon is a magnifying glass, with which he scrutinizes inscriptions in forgotten languages. Usually his triumphant decipherment coincides, in the last chapter with the daughter's rescue from savages by the handsome young assistant.

Kidder concludes that there are, in reality, some of each kind, but that neither the treasure-hunter nor the epigrapher attains the goals of archaeology.

The public's image or images of the archaeologist, his work and his goals, are not wholly trivial. Those unfortunate enough to have witnessed a site cluttered with telltale holes might have reflected upon the motive of those responsible. Somewhat dependent upon one's disposition, words like curiosity, treasure-hunting, ignorance, or wanton destruction might account for the circumstance. Clearly, motivation will differ from case to case. One might, however, ask a further question, and perhaps a more important one: What images must the public have of archaeology to thus treat its data?

It would be difficult to find whether Kidder's impressions about public belief are correct without Roper-like machinery. Another approach, however, is open. If it is granted that a major source of learning is via mass communications, it might be possible to consider at least some of what the public has an opportunity to learn about archaeology. Such an approach cannnot yield what the images are, but they might provide a glimpse of what goes into formulating them.

Curious as to what the results of such an approach might be, I read every article on archaeology in *Life* magazine in the ten-year period 1946–55 in an attempt to identify major themes. *Life* was chosen for two reasons. It has the largest circulation of any weekly magazine and it prints a relatively large number of articles about archaeology. According to *A Study of Four Media*, conducted for *Life* by Alfred Politz Research, Inc., *Life* attained in one quarter of 1953 a cumulative audience of 73,050,000 people, over 60 per cent of the U.S. population. In the ten-year period considered, *Life's* semi-annual index listed 34 titles under archaeology of which 33 are pictorial essays of the type peculiar to *Life*. Twenty-three are concerned with Old World archaeology, of which three, 16, and four, can be considered Biblical, Classical, and Prehistoric, respectively. Eight articles deal with the New World, three with Mesoamerica, two with the Paleo-Indian, and two with the Southwest. The remaining three, although concerned with particular times and places, are largely methodological. One is about underwater archaeology (July 27, 1953), another about areal photography (June 25, 1951), and a third concerns the construction of a model from the Tepexpan skull (November 3, 1947).

It was possible to identify four major themes.

Theme 1. The way of discovery. In more than half of the archaeological finds reported, the discoverer is not an archaeologist. The non-professionals include a housewife (June 16, 1951), a sand salesman (September 19, 1955), schoolboys (February 24, 1947), a village priest (December 6, 1948), a professional photographer (November 21, 1949), farmers (September 5, 1950), a group of Sunday spelunkers (October 20, 1952), shepherds (June 15, 1953), and a pipeline welder (July 12, 1954). In those cases where archaeology is planned, the particular recovery reported is often unwittingly stumbled upon. Archaeologist Zakaria Ghoneim, for example, falls headlong into an Egyptian tomb (June 21, 1954) and Coon is so surprised at seeing skeletal remains at the base of Hotu Cave that he collapses (May 21, 1951). Discovery appears to be a matter of chance. Since this is so, anyone is capable of discovery and the non-professional may participate in the grand adventure.

Theme 2. The expert called in. Adventure exits after the initial discovery is made. In some few cases, as for example, when the site is opened to public digging at the price of one dollar per pit (January 21, 1952) or when a town digs a Lake Dweller site (January 26, 1953), the community becomes archaeologist without professional aid. More often, however, it is at this point that the expert enters. Thus, the British Museum excavates at Sutton Hoo (June 16, 1951), and the Dominicans piece together biblical fragments (June 15, 1953). The expert's job, if little else, is depicted as time consuming. The British Museum's task lasted twelve years, and the Dominicans will have to labor for ten.

Theme 3. The techniques in detail. It was noted that three of the 34 articles are devoted to method. Concern with how things get done are not limited to these instances. *Life* devotes much attention to instructing its audience on the details of baking tablets in an oven in order to be able to clean them sufficiently to read (March

15, 1948), on how steeple jack-like methods must be used to get into position to make casts of the Darius inscriptions (May 23, 1949), and how wet sponges are used to soften age-brittled parchment fragments (June 15, 1953). Most of these procedures are described in a series of detailed pictures.

Theme 4. The firstest with the mostest. The modifiers which *Life* employs in describing finds are chiefly superlatives. Discoveries are the largest, as in the case of a Greek vessel (August 2, 1954), the cheapest, as in the case of the $8.65 paid to stone cutters to open the way to an Egyptian boat (June 14, 1954), or the earliest, as in the case of the Midland skull (July 12, 1954).

It would be unfair to leave this brief thematic analysis without mention of *Life's* contribution. This contribution will be obvious to anyone who has ever paused long enough to glance through *Life*. The full-page color reproductions of the Bonampak murals (November 21, 1949), for example, are unsurpassed in scholarly and popular journals. *Life* goes further than this. Its pictures of the Montignac cave paintings (February 24, 1947) and those of the early Christian vaults below St. Peter's Cathedral (March 27, 1950), are original documentary contributions.

Conclusions

Life's coverage of archaeology spans a wide range in both space and time. This is more likely due to chance than to any concerted effort. Since the selection of what is to be printed is probably based on what is newsworthy, it is not surprising to find heavy use of superlatives. It would not be unreasonable to conclude that a reader of *Life* might form the impression that the ultimate aim of archaeology is the discovery of the earliest, the biggest, the most.

Life's concern with how things get done is also a case of selection. Here the selection involves the realization by the magazine's writers that its audience might be more concerned with the technology of science than with its ideology. Emphasis is centered upon objects and not what historical and cultural inferences might be based upon them. Since technique is always depicted as the special alchemy of the expert, it might be concluded that to be an archaeologist, is to have knowledge of technique.

The dissociation of archaeological discovery from the expert work which follows it, integrates well with both the presentation of archaeological goals and what it is to be an archaeologist. As has already been pointed out, anyone can make a discovery since it is largely a matter of chance. The discoverer achieves both the goal and the adventure of archaeology. The professional may achieve both insofar as he makes the find, but he must share a place in the sun with the public. To be an archaeologist, more often than not, is to be an expert technician on call.

Life was chosen for analysis because of its large circulation and the number of articles on archaeology that appear in it. No claim is made that archaeology as depicted by *Life* is representative of other mass media or even of similar media. In fact, a cursory examination of some archaeological articles in the more sophisticated magazines like *The New Yorker, Atlantic Monthly,* and *Harper's* suggests a number of other possible and even contradictory themes. "Pot Hunters" which appeared in *Harper's* (August, 1954), for example, emphasizes the cooperative role that the public can play in the archaeological endeavor. A *New Yorker* "Reporter at Large" article (October 30, 1954) follows an excavation in London and focuses on careful excavation, scholarly interpretation, and public enthusiasm.

Fiction is clearly as amenable to analysis as non-fiction. Here, the depiction of archaeology may be expected to vary widely both between and within media. Humor in the form of cartoons and articles might also be scrutinized. An example of how skillfully and sympathetically archaeology can be treated in fiction may be found in Oliver LaFarge's recent story in the *New Yorker* (January 10, 1959). H. F. Ellis' "Letter to Posterity" in the *Atlantic Monthly* (November, 1955) in which the author imagines various ways his skeleton and ballpoint pen might be interpreted by an archaeologist in A.D. 20,000 is an example of how entertaining archaeological spoofing can be.

If mass communications contribute to the formation of the public images of archaeology, it might be useful to consider more fully the information they dispense. This is especially true for a science so often dependent upon public cooperation.

University of California
Los Angeles, Calif.
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THE VEHICULAR SLIP

James B. Shaeffer

Abstract

The small agricultural slip formerly used with horses or mules for dirt removal on farms and railroads is an efficient and inexpensive device for two or three men with a pick-up truck to use for moving back dirt.

ONE OF THE PRINCIPAL excavation problems for the small archaeological party is the disposal of the dirt resulting from the dig, especially when large areas are opened. If sufficient funds are available, dirt moving machines are always preferable as a time-saving method. When insufficient funds or other circumstances prevent the use of machinery and sufficient time and labor is available, dirt may as easily be removed by wheelbarrow or other containers. However, much excavation is conducted under circumstances which preclude these optimum conditions. Then some other means of dirt removal becomes desirable.

There are several requirements for mechanized dirt removal on the average small dig. The means must be economical to purchase and operate; it should be easy to transport by pick-up truck or station wagon; it should occupy a minimum amount of storage space; and efficient

operation should require not more than two persons. While there are probably other methods which fill these requirements we have found that the use of the agricultural "slip" meets these needs admirably.

The slip is simply a large flat-bottomed steel scoop with sides which taper forward to form a triangular outline in cross section. Attached to the sides of the slip by large metal eyes is a movable wishbone-shaped yoke. The chain or cable which hauls the slip is fastened at the apex of this yoke. These slips were in common use on farms and railroads when horses and mules furnished the principal motive power for dirt removal. Today they are often hard to locate, but inquiries in rural areas and at railroad yards may eventually turn one up. They were manufactured in a number of shapes and sizes from a heavy 900-pound, double-width slip with three or more handles to a small slip 30 inches wide and weighing around 300 pounds. This latter type is suitable for the small dig (Fig. 1). Since much of the total weight is actually concentrated in the steel yoke, it might be desirable in some cases to modify the yoke attachment by substituting bolts for the riveting so that the yoke could be separated from the slip for proper stowage. A small used slip will probably cost from $5.00 to $15.00.

The key to the efficient use of the slip lies in the length and weight of the cable or chain used. If the chain is too long, traction is lost; if it is too short, the slip becomes less efficient as it cannot be "swung" far enough away from the vehicle to scoop up dirt off to the sides. It has been found that a 9-foot length of ¾-inch chain will do the average job. In our case the motive power was a four wheel drive Willys station wagon. The chain was given a single wrap around the rear bumper and trailer hitch. It is probable that a pick-up truck or standard station wagon would do as well since control rather than power is essential for safe and careful operation.

Fig. 1 [Shaeffer]. A small agricultural slip used for dirt removal on archaeological projects in Oklahoma.

In operating the slip it has been found that dirt removal progresses best when a simple circular route can be established between the dirt pile and the dump area. This not only eliminates much difficult maneuvering of the slip by its handles but saves the time consumed in backing the vehicle into position. In this routine the slip is placed upside down while the vehicle approaches as close to the dirt pile as possible. The object is to place the vehicle in such a position that the slip can be swung on its chain with sufficient freedom to scoop up dirt behind the vehicle and slightly to one side. It has been found that it is easier for the driver to control his approach and pickup if the slip is swung to the left, or driver's side, where he can watch the operator.

When the vehicle is in approximate position it is stopped on signal by the slip operator who grasps one handle and pulls the scoop over backward into operating position. The vehicle then slowly moves forward. At this stage the operator must actively guide the slip, often moving it up and down by its handles so that a full load is scooped. The driver must also be extremely alert for if stones, a dip in the ground, or any other obstacle impedes the slip, it will dump itself while the sudden jolt is quite apt to catch the operator unaware and possibly throw him.

At the dumping area the driver must slow the vehicle when the operator is about to lift the handles to dump the load because the slip may occasionally fail to dump. This may cause the operator to lose his footing momentarily or he may stumble in the debris of the previous dump. Therefore the driver should be ready to brake his vehicle instantly to avoid an accident. A slow steady pace here also makes it easier for the operator to lift and dump the slip. If more than two persons are available, it expedites matters to have a third person stationed as dump operator. Rather than dumping in a large mound, a better method is to dump the dirt in a line several feet wide. In this way the dump can be easily picked up later for back fill by simply going down the length of the dump and pulling dirt off to the right or to the left.

The main advantage of the slip over heavy machinery is that it is always under the control of the archaeologist. Large machines, because of their size or the expense of operation, are often borrowed or rented for definite periods. However, with a vehicular hauled slip available at all times, dirt can be removed as required, making for better control. On a small dig with limited personnel it has been found that half an hour's work by two men working with a slip behind a vehicle either at the beginning or at the end of a day's dig usually takes care of the back dirt situation.

There are some limitations to the use of the slip as an all around piece of field machinery. The main disadvantage is that the slip is not a good means of taking off top soil due to its propensity to dump whenever subsurface obstacles are hit. In some soils it might skim the overburden satisfactorily but in most situations control of the slip would be too difficult for the operator. For refilling excavated areas heavy machinery such as bulldozers, road patrols or tractors with blades are more satis-

factory because of their size. The fact that one man operates the vehicular towed slip limits its size, making it too slow to be efficient as a back filling machine. Nevertheless, it is as a field expedient for shovel replacement rather than as a substitute for real machinery that the slip fills a need. More power machinery is undoubtedly the answer to the weary archaeologist's prayer, but until that lucky day arrives, the vehicular hauled slip is a compact, cheap, efficient, muscle saving substitute for the long handled shovel.

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BLADE MANUFACTURE IN SOUTHERN CALIFORNIA

B. K. SWARTZ, JR.

ABSTRACT

The striking of long, slender, parallel-sided flakes, or blades, from prepared cores has long been known in the Arctic and Mesoamerica. Small pointed blades with distinctive triangular cross section are also found in the Late Horizon in both mainland and island Canaliño sites in that part of the Santa Barbara Channel region of southern California which was ethnographically occupied by the Chumash.

MUCH HAS BEEN WRITTEN in recent years about a blade manufacturing technique which occurs in the New World, particularly in the Arctic. The purpose of this note is to call attention to the use of this technique in the Santa Barbara Channel region of southern California (Swartz 1959).

Bryan (1950: 5, footnote) and Solecki (1955) have pointed out the confusion surrounding blade terminology. In the New World the term *blade* refers to artifact use. All implements analogous in form to our technological concept of a knife possess "blades." A label that imputes a particular use to an artifact type can be dangerous. Further investigation may reveal that the imputed use is not valid. In the Old World the term *blade* is based on technique of manufacture. In other words, a blade is a specialized flake struck from a prepared platform. The latter definition is followed in this paper and the term *blade* is restricted to relatively long, slender, parallel-faceted flakes. It may be difficult to recognize blade manufacture since blade facets may be obliterated by secondary chipping, especially in larger specimens.

Blade manufacture has certain advantages. It is a simple process for the production of pointed or sharp-edged tools and can be applied to implements of microlithic size. Manufacturing can be performed rapidly and with little physical effort. For these reasons it is not surprising that blade manufacture also occurs in the New World where the use of prepared cores was widespread.

Two main blade manufacturing regions have been recognized in North America: the Arctic and Mesoamerica. In the Arctic, blade manufacture was first described by Nelson (1935: 356). These blades are produced by directing a blow on the striking platform of the core just anterior to the juncture of the two previous channel scars, detaching a blade with an obtuse isosceles cross section (Fig. 1 *a*). The blades seldom exceed 5 cm. in length and are manufactured primarily from chert. Circular and oval platform cores are both present. Blade manufacture is widespread in the Arctic and occurs in the University of Alaska Campus (type site, Nelson 1937; terminology, Solecki 1950), Dorset (Leechman 1943: 370–1), and Greenland Paleo-Eskimo (terminology, Meldgaard 1952: 229; Knuth 1954) horizons. There is some evidence that this blade type extends into more southerly areas such as Hopewell (Mills 1921: 137–41, Figs. 20, 21) and Poverty Point (Haag and Webb 1953).

The second main blade manufacturing region is Mesoamerica. These blades are produced by directing a blow on the striking platform of the core parallel to the chord of the channel apex, detaching a blade with a concavo-convex cross section (Fig. 1 *b*). Mesoamerican blades are much longer than those from the Arctic, ranging up to 18 cm., and are manufactured from gray or green obsidian. Kidder (1947: 20) divides Mesoamerican blades into one utilitarian and two ceremonial types. Circular and thin-oval platform cores are both present. Torquemada's description of Mesoamerican blade manufacture is given by Kidder, Jennings, and Shook (1946: 135–6).

With the exception of a brief statement by de Laguna (1947: 172) no notice has been given to blade manufacture in southern California. Prior to de Laguna's observation blades were included in a category termed "drills" (Rogers 1929: 398–9; Olson 1930, Table 4) which also includes non-blade artifacts.

A total of 3389 microflakes were observed in collections housed at the Santa Barbara Museum of Natural History, the Los Angeles County Museum, and the Southwest Museum. The majority of the implements are from Skunk Point, Skull Gulch (Fig. 2), and Ranch House on Santa Rosa Island, and Cochie Prieto on Santa

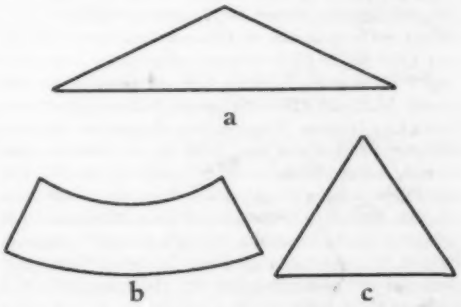

FIG. 1 [Swartz]. Idealized cross sections of North American blade types. *a*, Arctic; *b*, Mesoamerica; *c*, Southern California.

Cruz Island. Smaller numbers come from San Miguel and Anacapa islands and the mainland coast.

No cores were noted in these collections, but McKusick (1959, Fig. 3) recovered a cylindrical-shaped core from An*I*–8 (AS-UCLA) on west Anacapa Island. This lack of cores makes it difficult to deduce the manufacturing technique employed. The absence of cores may be explained (a) by their not being found (perhaps due to lack of investigation of factory sites) or recognized, (b) by their being classed as some other artifact type, or (c) by their being disposed of by the makers themselves.

The flat butts and body curvature of many of the specimens demonstrate that they are blades and were struck from prepared platforms. The outside portion of the blade, in relation to core detachment, is a facet; the inside, a ridge. In cross section some blades tend to be equilaterally triangular (Fig. 1 *c*). It is difficult to see how this cross section could be maintained by detaching blades from subconical cores, the core type used elsewhere in North America. Perhaps blades were struck from a sharp-angled, noncircular core such as a flat-butted flake, or by a bipolar technique.

Blades range in size from 1 to 5 cm. although Olson (1930; 1953, personal communication) discusses another size range, 7.5 to 20 cm., which he terms "picks." Tan chert is almost exclusively used as the manufacturing material.

In excavation, blades are often found in cemetery caches (Olson 1930; Orr 1953 and Harrison 1959, personal communications). Probably most of the blades are non-utilitarian burial offerings. This is further supported by the absence of any evidence of hafting, such as asphaltum which is the usual binding agent of this area, and the rareness of wear along the edges of the blades. However the Canaliño artifact inventory suggests that some blades did have utilitarian uses. The presence of beads, cylindrical tubes, pipes, circular fish hooks, and lashing holes in boat planks and steatite mortars shows that drilling must have been an important manufacturing process. Engraved abalone shell suggests another possible tool use. Blades of the smaller size range, if hafted, would be well suited for these purposes.

Rogers (1929) describes "drills" under his latest horizon, the Canaliño. Olson (1930) obtained the bulk of his collections from his Intermediate to Late Island horizon. Orr (1951: 224; 1953, personal communication) recovered his specimens from his Late period of Santa Rosa Island. McKusick (1959:86) groups Anacapa microblades in his Late Horizon. These datings suggest that Canaliño blades are recent and may prove to be a reliable time marker, supplementing others compiled by Eberhart (1957), for southern California archaeology. Blade manufacture appears to be restricted to areas ethnographically inhabited by the Chumash; it is not present in areas inhabited by Shoshonean speakers. Therefore blades may serve not only as time markers, but also space markers in southern California.

The occurrence of equilaterally triangular cross sections is a distinctive feature of southern California blades and indicates that a unique manufacturing technique was

Fig. 2 [Swartz]. Microblades from the Skull Gulch area, Santa Rosa Island, California (Loc. No. 131.2 SBMNH). Length of *a*, 5.8 cm.

practiced in this area. However, before detailed information on manufacturing technique can be obtained cores must be found. To date no carefully controlled excavation of a Canaliño site has been made.

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ANCIENT MESOAMERICAN MORTARS, PLASTERS, AND STUCCOS: THE PUUC AREA

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ABSTRACT

Chemical and microscopic examination of building materials from Uxmal, Kabah, Sayil, and Labná demonstrate the presence of all the building elements previously identified at Comalcalco, Las Flores, and Palenque: mortar, plaster, stucco, wash coat, and lime-aggregate. The building elements at these Puuc sites differ from those at Comalcalco and Las Flores in the use of non-marine sources of lime, greater strength, and generally smoother composition. They differ from the Palenque building elements in the lack of multi-laminate structure and the absence of very smooth and highly worked outer surfaces. It is possible that some form of lime other than that obtained by burning limestone was used. An inclusion of gypsum found at Uxmal is the only such component identified at any of the Mesoamerican sites studied so far.

ALL THE BUILDING ELEMENTS, mortars, plasters, wash coats, stuccos, and lime-aggregates, identified previously at other sites were found to have been used in the Puuc area. A single example of the use of a lime-aggregate extends still further the area in which this building element is found. Previous work disclosed its use at Comalcalco, Las Flores (Tampico), and Palenque (Littmann 1957, 1958a, 1959a, 1959b).

In view of the architectural similarity between sites in the Puuc area, it is of interest to determine if there was also a similarity in building techniques as reflected in the building elements. To this end, samples were collected in 1956 and 1957 from Uxmal, Kabah, Sayil, and Labná. While the greatest number of samples were taken at Uxmal, those from the other sites were felt to be adequate in number to demonstrate any significant differences or similarities between the sites. In attempting to compare samples, it is immediately apparent that only major similarities or differences can hope to be established. The prolonged and non-uniform exposure of surface coatings, mortars, and such to the atmosphere and soil, precludes an interpretation of detailed chemical analyses. Because of the known use of *sascab* in making building elements in Yucatán, and because the composition of *sascab* often differs greatly from its associated limestone, it is frequently impossible, from chemical analyses, to identify the lime used in the building element with its obvious source (Littmann 1958b).

The building elements of the Puuc area (Table I) dif-

TABLE 1. SUMMARY OF ANALYSES OF PUUC BUILDING ELEMENTS.

Site and Number	Structure	Classification	Acid Insoluble %	Ca %	Mg %	Acid Insoluble-Free Basis Ca %	Acid Insoluble-Free Basis Mg %	Color	Ca/Mg Molar Ratio
UXMAL									
1	Templo del Adivino — Temple I	Wash coat	2.9	34.9	3.4	35.9	3.5	White	6.2
1a	Templo del Adivino — Temple I	Plaster	4.1	37.7	2.1	39.3	2.2	Pink	10.9
2	Templo del Adivino — Temple I	Wash coat	1.2	36.8	2.1	37.2	2.1	White	10.5
2a	Templo del Adivino — Temple I	Plaster	3.6	32.7	4.7	34.5	4.9	Pink	4.2
3	Templo del Adivino — Temple I	Mortar	4.7	26.1	9.2	27.4	9.7	Pink	1.7
4	Templo del Adivino — Temple II	Mortar	9.6	4	6.1	5	6.8	Pink	2.9
5a	Templo del Adivino — Temple II	Plaster	6.4	29.0	6.7	31.0	7.2	Pink	2.6
6	Templo del Adivino — Temple II	Mortar	7.4	29.8	6.1	32.5	6.8	Pink	3.3
6b	Templo del Adivino — Temple II	Gypsum	0.5	28.6	0.29	28.8	0.29	White	—
7	Templo del Adivino — Temple III	Wash coat	2.7	33.3	3.2	34.2	3.3	White	6.3
7a	Templo del Adivino — Temple III	Plaster	7.3	28.8	4.8	31.1	5.1	Pink	3.6
8	Templo del Adivino — Temple III	Mortar	5.3	30.7	4.9	32.4	5.1	Pink	3.8
9	Templo del Adivino — Temple IV	Plaster	6.0	35.7	1.1	38.0	1.2	Pink	19.0
10	Templo del Adivino — Temple IV	Mortar	6.4	33.5	2.6	35.8	2.8	Pink	7.6
10b	Templo del Adivino — Temple IV	White inclusion in 10	0.4	37.2	0.68	37.4	0.68	White	—
11	Templo del Adivino — Temple V	Mortar	7.1	32.8	2.7	35.3	2.9	Dark pink	7.3
12	Templo del Adivino — Temple V	Lime-aggregate	44.2	17.9	1.9	32.1	3.3	Brown	5.8
12a	Templo del Adivino — Temple V	White inclusion in 12	2.8	35.2	1.9	36.2	2.0	White	11.0
13	Nunnery, East Building	Wash coat	1.3	31.7	2.1	32.1	2.2	White	9.0
13a	Nunnery, East Building	Plaster	3.0	34.0	1.8	35.0	1.9	Pink	11.0
14	Nunnery, East Building	Mortar	3.3	33.5	2.1	34.6	1.5	Pink	9.7
15	House of the Pigeons	Plaster	5.0	36.9	1.5	38.8	1.6	Buff	14.5
16	House of the Pigeons	Mortar	6.6	31.1	4.7	33.3	4.9	Buff	4.1
17	House of the Pigeons	Stucco	2.5	37.3	1.1	38.3	1.1	Buff	20.7
KABAH									
1	Headquarters Building, Northwest Room	Wash coat	8.4	32.0	4.6	34.9	5.0	Dark buff	4.2
2	Headquarters Building, Northwest Room	Plaster	6.0	35.1	2.1	37.4	2.2	Buff	10.2
3	Headquarters Building, Northwest Room	Mortar	6.9	32.1	3.3	34.5	3.5	Light buff	5.9
4	Headquarters Building, Southwest Room	Plaster	7.2	35.5	1.5	38.2	1.6	Light buff	14.2
5	Headquarters Building, Southwest Room	Mortar	5.5	35.7	0.73	36.8	0.77	Cream	28.7
6	Temple of the Columns	Plaster	3.5	35.3	1.0	36.6	1.0	Dark buff	22.0
7	Ramp to Arch	Upper layer	3.4	36.5	0.98	38.1	1.0	Buff	22.9
8	Ramp to Arch	Intermediate layer	3.7	36.8	1.3	38.2	1.4	Buff	16.4
9	Ramp to Arch	Lower layer	5.9	35.6	0.98	37.8	1.0	Cream	22.7
SAYIL									
1	Palace, West Room, 2nd Level	Plaster	6.1	35.7	1.2	38.0	1.3	Light buff	17.5
2	Palace, West Room, 2nd Level	Mortar	6.0	35.8	1.4	38.1	1.5	Light buff	15.2
3	Palace, North Exterior Wall of Top Building	Plaster	5.8	36.2	0.86	38.4	0.91	Buff	25.3
4	Palace, West Arch — South Wall of Top Building	Mortar	7.0	36.3	0.74	39.0	0.79	Light buff	29.6
5	Chultun	Plaster	29.9*	16.7†	3.5†	24.2	5.1	Red	2.8
6	Chultun	Mortar	0.77	39.1	0.49	39.4	0.50	Cream	47.4
LABNÁ									
1	Group North of Caretaker's House	Plaster	7.3	28.1	1.9	30.3	2.1	Buff	8.7
2	Group North of Caretaker's House	Mortar	7.2	35.0	0.98	37.3	1.1	Light buff	20.3

* Based on treatment with dilute acid

† Based on solution of sample in concentrated HCl

fer in several ways from those collected at Comalcalco (Littmann 1957, 1958a) and Las Flores (Littmann 1959a), chiefly because they are made of non-marine sources of lime, their greater strength, and generally smoother composition. They are markedly different from similar elements at Palenque (Littmann 1959b) in that there is no evidence of a multi-laminate structure and an absence of the very smooth and apparently highly-worked outer surfaces. The use of wash coats seemed also to be less frequent in the Puuc area. The rather widespread use of granular material and stone chips in the mortars and plasters in the Puuc region were also distinguishing characteristics and probably account for the weaker and more friable nature of some of the samples when compared with functionally similar materials from Palenque.

The presence of small stones, sand-like in appearance, along with larger and often rounded gravel-like stones, stone chips as well as rather soft, white inclusions, suggested the general use of a form of lime other than that obtained by burning limestone. These observations, particularly with reference to the white inclusions, are a confirmation of earlier ones by Holmes (1895: 155–6) who, in writing about Palenque, states:

> . . . there is here, as in Yucatan, little admixture of sand with the lime. . . . Lime is everywhere and the chief ingredient, though sand has been used in some cases as well as coarser materials. . . .

In reporting upon samples submitted by O. C. Farrington for examination, Holmes presents these comments:

> No. 3, from Cozumel, was yellowish-white, porous, friable, and granular, and contained lumps of pure white, more compact material.
>
> No. 4, from Uxmal, was similar to No. 3 in color, tenacity and texture, but contained also harder and darker grains.
>
> On grinding some of the limestone of the region (Uxmal and Cozumel), mixing it with water, and allowing it to dry, it was found to set to a firm mass. It is probable, therefore, as the appearances of Nos. 3 and 4 indicate, that material of this character was mixed with lime for making mortar.

The white inclusions referred to by Holmes were also observed in the present work. However, one of these inclusions, found in the mortar of Temple II, Temple of the Dwarf, at Uxmal, proved to be quite different from all others in that it was shown to be gypsum. No previous reference to the occurrence of gypsum as a component of mortars or plasters has been found. The significance of its presence is unknown at this time.

In order to establish the use of an area-wide building technique in the Puuc area, it is necessary to examine the common use of similar raw materials and to take into account any localized variations of these materials. A review of the characteristics of the samples collected leads to the conclusion that their similarities are probably not accidental. Of the 36 samples of true building elements examined, only eight showed the presence of carbon. The absence of carbon may imply the absence of burned lime, or its use in very limited amounts in the element as applied, or a method of burning different from the traditional one, using a pyre of radially placed logs. The former condition appears the more likely in the absence of any references to unusual lime-burning methods.

The widespread presence of the calciferous, sand-like particles referred to earlier, stone chips, and gravel-like, rounded stones in most mortars and some plasters appears also to be significant as does the white inclusions.

While it is probably impossible to demonstrate physically or chemically, the colors of the building elements appear to be related to the *sascab* available in the immediate area. Pink and white mortars and plasters are found at Uxmal where red, yellow, and white *sascab* types are available. The buff-colored mortars and plasters of the other Puuc sites are generally of a reddish tinge, possibly related to the red *sascab* found at Sayil.

The presence of the sand-like and gravel-like stones, the white inclusions, the general absence of appreciable carbon, and even the colors of the building elements point to a single technique in their preparation in the Puuc area. It seems highly probable that the mixtures used for mortars, plasters, and stuccos were composed of *sascab* or a similar material, possibly taken from stream beds which are dry washes during a portion of the year, stone chips, the by-product of quarrying or stone-dressing operations and only sufficient burned lime to facilitate rapid setting. The observations of Holmes regarding the behavior of ground limestone and water and the duplication of these results with several *sascab* types and water lend strength to the conclusion that only minor amounts of burned lime were necessary to secure satisfactory mortars and plasters. This situation probably does not apply to wash coats. While it is also possible that plant extracts were, in some cases, applied to the surface or mixed with plaster to obtain a stronger product, this point has not been proven.

The use of readily available, finely divided aggregate in the preparation of building elements is certainly an "economic" process in terms of the labor required. It was used extensively at Comalcalco and Las Flores (Tampico) and to a limited extent at Palenque where the elements were designated lime-aggregates since they contained large quantities of non-lime material such as earth or sand. While the mortars and plasters may have been made by the same technique as for lime-aggregates, neither their composition nor function would permit such a classification. In Uxmal Sample No. 12, however, there is both a composition and use in the construction of the pyramid of Temple V, Temple of the Dwarf, which permit classification of the element as a lime-aggregate, undoubtedly another example of the same technique, but with different raw materials, applied throughout the Puuc. Yet another example of the suggested technique is found in Sayil Sample No. 5 in which the non-lime portion of the material was a mixture of iron oxide and a silicate (probably a crude iron ore) which, because of its function, was classified as a plaster.

On-Site Observation and Sample Descriptions

UXMAL

Temple of the Dwarf (Templo del Adivino)

Temple I

Numbers 1, 1a. A sample of wall coating 20 to 30 mm. thick was removed from the interior wall of the interior room. It consisted of a white exterior wash coat 0.6 mm. thick applied over a

heavier tan plaster which contained colored sand-like particles. The layers are so tightly bonded as to suggest the application of the wash coat before the plaster was dry or had been applied as a thin slurry. As removed, the sample showed little tendency to crumble, though once the surface was broken it crumbled readily and showed little cohesion. The wash coat was very fine textured and was removed by scraping. The softness of the plaster suggests the use of a mixture poor in burned lime as does the inclusion of the sand-like particles which were largely soluble in dilute hydrochloric acid. No carbon was found in either sample. *Classification*: 1, wash coat; 1a, plaster.

Numbers 2, 2a, 3. The samples obtained from the façade were a wall coating applied over a mortar between the columns and the mortar. The coating was 7 to 8 mm. thick and consisted of a wash coat less than 0.5 mm. thick which could be isolated in small pieces by carefully scraping away the softer plaster. The wash coat was nearly white, but appeared to have been painted red. The plaster was somewhat finer in texture than Sample 1a and also contained sand-like particles. The mortar was soft and friable and, in addition to the sand-like particles, contained stone fragments. The stone was almost pure white and was removed before the mortar was analyzed. The mortar also contained stone-like particles of the same color as the mortar and similar to the lumps found in red Uxmal *sascab*. No carbon was found in any of the samples. *Classification*: 2, wash coat; 2a, plaster; 3, mortar.

Temple II

Number 4. A sample of mortar from the pyramid was buff-colored, very soft, and crumbled on handling. It also contained stone fragments which were removed by screening through window screen before analysis. *Classification*: 4, mortar.

Numbers 5a, 6, 6b. These samples, taken from the wall of an interior room, consisted of a wash-coat-covered plaster covering mortar between the stones of the wall and the mortar. The 0.2 to 0.5 mm. wash coat was very friable and appeared to have been brushed on. It was buff-colored and may have been painted or treated with a plant juice such as *chucum* or haematoxylin extract. It could not be separated for analysis. The underlying plaster was about 10 mm. thick, similar to Samples 3 and 4, and exceedingly friable and crumbly. The mortar was similar in appearance to Samples 3, 4, and 5a but was a mixture of powdery buff-colored material and a relatively hard white substance which, however, was not stone-like in appearance. The two substances were readily separated by screening through window screen. The white substance, while part of the mortar, has been listed separately as a sample since it was found to be gypsum. *Classification*: 5a, plaster; 6, mortar; 6b, gypsum.

Temple III

Numbers 7, 7a, 8. The samples taken from the wall of the temple were a wash-coat-covered plaster 25 to 30 mm. thick and mortar from between the stones. The wash coat was about 1 mm. thick, white, and stronger than those found in Temples I and II. It also had a somewhat coarser texture but showed no evidence of painting and was readily separated from the plaster. The plaster was buff-colored and contained a large number of sand-like particles, some pieces of stone as large as 5 by 15 mm., and a few particles of carbon. The mortar was light buff in color, fine-textured, soft, and contained stones similar to those found in the plaster. Other rounded stone-like particles, different in appearance from limestone fragments, were also present and, when broken, had an internal color similar to red *sascab*. No carbon was found. *Classification*: 7, wash coat; 7a, plaster; 8, mortar.

Temple IV

Numbers 9, 10, 10b. The unlaminated plaster taken from the wall of the temple was 4 to 10 mm. thick and had been painted pink. It was fine-textured and contained sand-like particles as well as some pure white material which suggested the addition of *sascab* to the buff-colored plaster. The white particles appeared softer than the buff-colored or sand-like materials. A sample of buff-colored mortar was removed from between the stones. It also contained sand-like particles, a soft, white material as in the case of the unlaminated plaster, and stone chips. The white inclusions were separated manually from the remainder of the mortar. The absence of a positive test for sulfates established the absence of gypsum. Even though some emphasis has been placed upon the presence of the white inclusions in the above samples, this does not represent a unique appearance of this material. The emphasis arises rather because of the quantity present and its ease of isolation. No carbon was observed in the samples. *Classification*: 9, plaster; 10, mortar; 10b, white inclusions.

Temple V

Numbers 11, 12, 12a. The materials collected from this temple were a mortar from between the stones of the temple itself and mortar from the pyramid. There is some question as to whether the temple was originally plaster covered. The temple mortar was buff-colored and contained stone chips and hard bumps similar to those described under Sample 3 which were screened out before analysis. The pyramid mortar was brown in color and appeared to be a mixture of earth and lime containing lumps of essentially white lime. The brown material crumbled easily but the white lumps were quite hard. Some of the white lumps removed by screening were as large as 50 by 50 mm. and on fracture looked as if the original material had been mixed with fine fibers. Under the microscope, the material had a lacy appearance and seems to have been subjected to appreciable leaching and possibly some recrystallization. Small white particles were also present in the sample taken for analysis. Some carbon was found in the temple mortar though none was observed in the pyramid mortar.

The classification of the material between the stones in the pyramid as mortar is probably not correct. While the stones of the pyramid face were shaped, they were not smoothly finished, nor were they fitted together as would be expected in the case of a building. The regularity of their placement in both vertical and horizontal rows and the large amount of earth-like material back of them suggest rather strongly that they were imbedded in a soft wet medium and that the spaces between them were later filled in with the same material. Because of these observations, the bonding material has been classified as lime-aggregate in accordance with the previously established system. *Classification*: 11, mortar; 12, lime-aggregate; 12a, white inclusion.

Nunnery

East Building

Numbers 13, 13a, 14. The samples collected from the interior wall were a laminated plaster and mortar from between the stones and under the plaster. The laminated wall coating was 3 to 5 mm. thick and was composed of an outer cream-colored layer 1 to 1.5 mm. thick applied over a buff-colored plaster. The outer layer showed traces of a very thin discontinuous coating which may have been paint or even a wash coat. The outer layer was also rough and sandy in appearance and showed some efflorescence, indicating it had been subjected to appreciable leaching. The bond between layers is not very strong, but the whole is strong enough to permit obtaining samples by scraping. The very thin paint coat could not be separated for analysis. Despite the possibility of inaccurate classification, the cream-colored coat has been designated a wash coat and the buff-colored one a plaster. No carbon was found in either sample. The mortar has a light buff color and contained some relatively large pieces of carbon as well as small stones, both of which were screened out before analysis. The mortar was appreciably softer than the wall coating. *Classification*: 13, wash coat; 13a, plaster; 14, mortar.

House of the Pigeons

Roof Comb

Numbers 15, 16, 17. The material collected consisted of samples of plaster, mortar, and stucco. The plaster was taken from the inside of the niche in the third comb from the west. It was 27 mm. thick, light buff in color, with a smooth outer surface which had been painted red. No wash coat was found, indicating that the plaster had probably been worked smooth upon application. Beneath the surface the plaster had a sandy appearance. A small amount of carbon was present. The mortar was taken from between the stones in the opening in the west wall of the arch and appeared to be a mixture of lime and small stones, many of which were rounded. Large pieces of carbon were also present. Both stones and carbon were removed by screening before analysis. The stucco was a piece found at the foot of the second comb from the west. It had a 28 by 56 mm. oval cross section and was about 60 mm. long. The piece was buff-colored, hard, and strong with a sandy appearance. No wash coat had been applied though traces of green paint remained. A second sample, molded as a rounded corner, was also collected and was similar in appearance to the other except that no paint was observed. Both samples contained carbon. *Classification*: 15, plaster; 16, mortar; 17, stucco.

KABAH

Headquarters Building

Northwest Room

Numbers 1, 2, 3. Wall coating and mortar were taken from the south wall. The wall coating was 7.0 mm. thick and consisted of a plaster covered with a wash coat about 0.5 mm. thick which showed definite "brush marks." Both plaster and wash coat were buff-colored though the latter was the darker and may have been painted or treated with a plant extract. While both layers are very hard, the wash coat is the stronger, nearly translucent, and of fine texture. It was separated from the plaster by scraping away the

latter with a knife blade and finally with emery paper. No carbon was found present. The plaster had a somewhat coarser texture than the wash coat and contained a large number of sand-like particles or very small stones. A small amount of carbon and some soft white inclusions were also present. The stones were removed as completely as possible by screening through cloth mesh with openings about half the size of those in window screening. The mortar was light buff in color, fairly soft and friable, but contained some hard lumps which were not stone. Some of the lumps were brown on the inside. A mixture of lumps and the powdery mortar was ground for analysis. No carbon was found present. *Classification*: 1, wash coat; 2, plaster; 3, mortar.

SOUTHWEST ROOM

Numbers 4, 5. A sample of plaster and mortar was taken from the south wall, the plaster from above the break in the arch and the mortar from between the stones. The 11 mm. thick plaster was light buff in color, weak and crumbly, and contained many small stones but no carbon. The sample was screened without grinding to remove the stones. No wash coat was found. The cream-colored mortar was quite soft and contained stones as large as 10 by 15 mm., but no carbon. Some non-stone lumps were yellow on the inside and were similar to lumps found in *sascab*. The sample was screened before analysis. *Classification*: 4, plaster; 5, mortar.

TEMPLE OF THE COLUMNS

NORTHWEST ROOM, LOWER LEVEL

Number 6. A sample of plaster was obtained from the interior west wall. It varied in thickness from 5 to 25 mm. and was dark buff in color. No wash coat was used, but the outer surface appeared to have been painted or treated with a plant extract since it was much darker than the other portion of the plaster and showed distinct "brush marks." The plaster was hard, strong, and fine-grained, and contained a large number of very small stones which were screened out after light grinding of the sample for analysis. No carbon was found present. *Classification*: 6, plaster.

ARCH RAMP, EXTERIOR

Numbers 7, 8, 9. The sample (courtesy of G. F. Ekholm) was a part of the floor leading to the ramp and was composed of three layers with an over-all thickness of 25 mm. The bottom layer was about 15 mm. thick, cream-colored, and fairly hard. It also contained stone chips as large as 20 by 40 mm. The upper layer was about 10 mm. thick and buff-colored. In other respects it was similar to the bottom layer except that it was free of stones. The two layers were separated by a very hard, thin, semi-translucent layer similar to one observed in a stucco at Comalcalco (Littmann 1958). It was sufficiently strong so that it could be freed from the other layers by brushing with a wire brush and finally grinding on an emery wheel. No carbon was found in any of the samples. Since the sample was a portion of a floor, no classification was assigned to the individual layers.

SAYIL

PALACE

WEST ROOM, SECOND LEVEL FROM TOP

Numbers 1, 2. The materials collected were a plaster and a mortar from the west interior wall. The plaster was 8 mm. thick, light buff in color, fine-grained, hard, and fairly strong. Its outer surface was smooth as if it had been polished or worked and was painted light brown. Again, there is the possibility that plant extracts had been applied to facilitate smoothing. No wash coat had been applied. The plaster was also free of carbon though it contained many small stones which were screened out before analysis. The mortar had the same color as the plaster but was softer and was mixed with stones as large as 20 by 40 mm. No carbon was found present. After drying, the mortar was easily powdered and any remaining lumps were broken between the fingers. *Classification*: 1, plaster; 2, mortar.

TOP BUILDING

Numbers 3, 4. A sample of wall coating from the north exterior wall, and of mortar from between the stones of the west arch in the south wall, were collected. The coating was 3 to 6 mm. thick, buff-colored, very hard and strong, and was covered with a dense wash coat about 0.5 mm. thick which appeared to have originally been painted. The wash coat was somewhat weathered and in places almost translucent. It was so firmly bonded to the plaster it could not be cleanly separated for analysis. It was ground away from the plaster before the latter was analyzed. The plaster itself contained small, rounded stones and white stone chips but no carbon. The mortar was light buff in color, soft and crumbly, and any lumps present could be broken between the fingers. Stones as large as 5 to 10 mm. were present but no carbon. *Classification*: 3, plaster; 4, mortar.

CHULTUN

Numbers 5, 6. Adjacent to one of the roads leading from Sayil to Labná is an opening in the ground designated locally as a *chultún*. While time did not permit a detailed examination of the structure, a brief description is presented because of one or two unusual features. The structure was essentially circular and entrance was made through a large opening in the side of the upper portion which also appeared to have a vaulted roof. The bottom was filled with earth and debris. The exposed, inner wall was red and near the top contained a molded human figure estimated to be 50 cm. or greater in height. The surface coating of the chultun consisted of a cream-colored mortar overlaid with an 18 mm. red plaster, which in turn was covered with a gray or buff wash coat less than 0.5 mm. in thickness. The wash coat could be removed from the plaster but was so thin and fragile it could not be properly cleaned for analysis. The red plaster was quite soft and contained a number of soft white inclusions, a few small stones, but no carbon. The plaster is quite unusual, as will be pointed out later, because of its depth of color which was due largely to a high concentration of iron oxide. No carbon was found. The mortar was appreciably harder and stronger than the plaster and was free of stones and carbon. *Classification*: 5, plaster; 6, mortar.

LABNÁ

FIRST BUILDING NORTH OF CARETAKER'S HOUSE

Numbers 1, 2. A sample of plaster and mortar were taken from the south wall. The plaster was very thin, 1 to 2 mm., buff-colored, and friable. It appeared to have been smoothed on and was free of any wash coat. Had this plaster been over another layer, it undoubtedly would have been classified as a wash coat. It appeared to have been unpainted but did contain a small amount of carbon. The mortar was light buff in color, quite crumbly, and free of carbon. It did, however, contain some small stones which were screened out before analysis. *Classification*: 1, plaster; 2, mortar.

LABORATORY ANALYSES

The analytical procedures employed were the same as those described in the initial paper of this series (Littmann 1957) except as noted below.

Samples rich in iron, or believed to be so, cannot be analyzed by the simplified procedure previously used without removing the iron completely. Furthermore, the iron, normally present as oxide, is included in the acid insoluble part since it is only slightly soluble in dilute hydrochloric acid during the short time it is exposed to it. The residue upon such a treatment may for all practical purposes be considered acid insoluble. It will certainly be free of calcium or magnesium except that which is combined in the form of clay or similar silicates.

To free a sample of iron for subsequent calcium and magnesium analysis, the following procedure was used. About 0.5 g. of material, accurately weighed, is treated with 10 ml. of concentrated hydrochloric acid and heated to 90 to 100° C. It is maintained at this temperature in a covered beaker until the residue is essentially white; 20 to 25 ml. cold water is then added and the solution filtered. The residue may be collected quantitatively and weighed if desired. The filtrate is made alkaline with concentrated ammonia, heated to boiling, and filtered. After cooling to room temperature, it is made up to volume (100 ml.) in a volumetric flask. One aliquot portion is reserved without further treatment for the magnesium determination. Another is acidified to litmus paper with hydrochloric acid and then made alkaline with 10% sodium hydroxide solution followed by an additional 5 ml. and boiled until the vapors are free of ammonia. After cooling, the solution may be titrated for calcium.

When gypsum is suspected, usually because of the presence of crystals upon cooling of the solution obtained by treating about 0.5 g. of sample with 10 ml. of 10% hydrochloric acid, the residue remaining from this treatment is heated with 10% ammonium chloride solution, slightly acidified with hydrochloric acid, until no more is dissolved. This solution is evaporated to dryness and the residue recrystallized from 5% hydrochloric acid. The crystals so obtained may be compared with authentic, recrystallized calcium sulfate and their solution in dilute hydrochloric acid tested for calcium and sulfate ions by usual methods.

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Esso Mexicana
Mexico 7, D.F.
August, 1958

A FLUTED POINT FROM HIGHLAND GUATEMALA

Michael D. Coe

Abstract

An obsidian fluted point was found on the slope of a gorge near San Rafael, just west of Guatemala City. The point probably washed from hills on the thick beds of volcanics through which the gorge is cut. This would confirm previous beliefs that the paleo-Indian occupation of the area post-dates the volcanics. The San Rafael point resembles the fluted points from Costa Rica and Durango, Mexico, and Clovis forms from the eastern United States. Thus, paleo-Indian influences from the north into Middle America may have been of predominately eastern rather than western origins.

WHILE PHOTOGRAPHING the excellent archaeological collection of Mr. and Mrs. Carlos Nottebohm in October, 1958, I came across an obsidian fluted point (Fig. 1). It had been discovered in December, 1956 by Carlos Nottebohm, Jr., while hunting pine cones with other children near San Rafael, a small resort about 12 km. directly west of Guatemala City. I had the opportunity to visit the location of the discovery a few weeks later in the company of Mrs. Nottebohm. The area, like most of this part of the Guatemalan highlands, is completely covered by an unbroken succession of volcanics hundreds of feet thick. A stream flows by San Rafael and down towards the town of Mixco, cutting through a deep *barranco*, or gorge, up which the road runs. The point was found off the road, on the south slope of the gorge. Although it cannot be proved, it seems likely that the point had been washed from the hills above, from an old land surface above the volcanic deposits.

The material from which the projectile point was made is banded black-gray obsidian of good quality, without inclusions. It presents a dull and slightly mottled surface, suggesting patination from age; a tiny fragment broken off the tip after discovery reveals a glossy facet of the interior. As can be seen in the accompanying illustrations, the point has a concave base, with fairly well developed "ears." The widest part of the point is near the middle. One side only has been fluted; the channel extends about half the length from the base. Evidently more than one attempt at fluting was made on this side, as the scar is double. The reverse side has been thinned at the base by flaking, but none of the flakes removed were as long as true flutes. There is fine secondary chipping all along both edges. Extremely cursory grinding appears on the base and edges up to about the middle of the point. Dimensions of the San Rafael point are as follows:

Actual length (from basal corners to broken tip)	57 mm.
Length from basal corners to end of reconstructed tip	59 mm.
Width of base	21 mm.
Width at widest point	27 mm.
Basal depth	3 mm.
Length of ground edges on either side	26 mm.
Length of fluting, from deepest point of base	24 mm.

The San Rafael fluted point represents the first surely paleo-Indian artifact yet found in Guatemala. The discovery of a fossil sloth bone in the Río Pasión drainage of the southern Petén which bore three probably man-made cuts may also be of great antiquity (Shook 1951: 93), if it is certain that the cuts were of the same age as the bone itself. The most significant result of the San Rafael discovery, however, is that it furnishes strong indications that at least part of the paleo-Indian occupation of this part of Middle America post-dates the huge deposits of volcanic eruptives. This has been suggested by Shook (1951: 93) as a likelihood, because Pleistocene mammalian remains have been found lying just above the latest volcanic ash bed in a Guatemala City suburb. In the future we can be certain that more finds of very early cultural material will be made in the Guatemalan highlands.

The San Rafael point is strikingly similar in shape and workmanship to the fluted point found in Durango, Mexico (Lorenzo 1953), the nearest fluted point known to the north. The Durango point has the same constriction above the base, with marked "ears," producing a "fish-tail" effect, and has the same kind of fluting. To the south, the fluted point from the Hartman collection from

Costa Rica (Swauger and Mayer-Oakes 1952) is also closely related in form and fluting to the San Rafael point, but lacks the strongly pronounced constriction and the "ears."

Further afield, all three points — Durango, San Rafael, and Costa Rica — would fit comfortably into the eastern Clovis series of fluted points known from Vermont to Alabama. There is a noteworthy resemblance of the San Rafael find to points from New York State figured by Ritchie (1957, Pls. 1 *a*, 5 *d*, 8 *b*, 10 *e*, and others) and to Clovis points in Tennessee and Alabama. It is quite possible that some of the earliest paleo-Indian hunters in Middle America had predominantly eastern rather than western origins, and crossed the Rio Grande from the southeastern part of the United States.

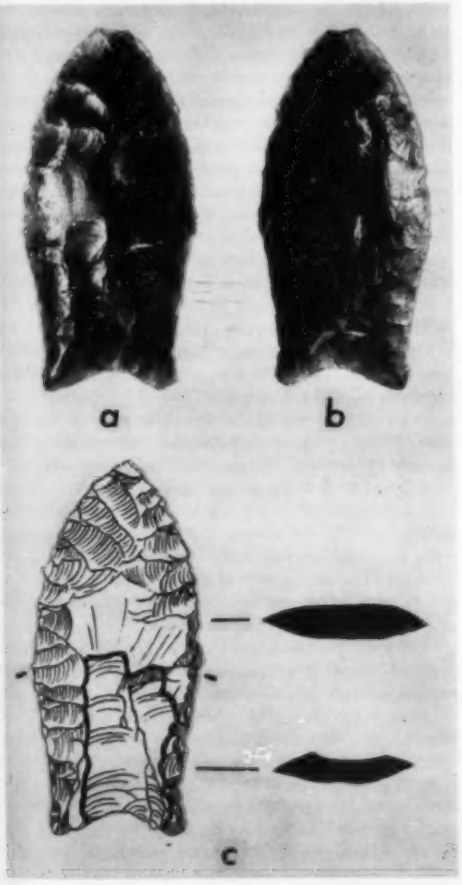

FIG. 1 [Coe]. Fluted point from San Rafael, Guatemala. *a*, obverse; *b*, reverse; *c*, drawing of obverse showing nature of fluting, sections, and extent of grinding on edges. Actual size.

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COMMENTS ON ALIBATES FLINT

F. EARL GREEN AND JANE HOLDEN KELLEY

ABSTRACT

Flints which were formed during different geological periods and which can be confused with Alibates flint occur in widely separated localities in west Texas and eastern New Mexico.

ALIBATES FLINT has long been thought to be recognizable and distinctive; it has generally been assumed that the flint came from the Canadian River outcrops, specifically the noted Alibates mines. This flint has usually been described as banded, with red and white or gray as the dominant colors. Shaeffer (1958: 190) has recently pointed out that a blue and white banded flint is also common at the mines, but since the blue and white is more difficult to recognize, it has not gained the popular designation of Alibates flint in archaeological literature. Alibates flint deposits occur in the upper Quartermaster beds of the Permian.

During the past few years, Green has had occasion to visit many locales in western Texas and eastern New Mexico in conjunction with geological research. During this time he has observed two occurrences of flint that closely resemble Alibates. The first locale is approximately 8 miles northeast of Silverton, Texas, where State Highway 256 crosses the Caprock escarpment, in which there are lenses of dark red and white mottled and banded flint. This flint was formed in the Dockum beds of the Triassic period. The second locale is some 10 miles south of Yeso, New Mexico, on Yeso Arroyo. At this site, the flint occurs in boulders that form a part of the Permian beds.

Both of the above localities produce flint that is well within the range of the red and white Alibates. This is true, not only of the coloration and visual impression, but in the quality and fracturability of the flint.

There are additional sources of flint that fall within the range of the less well known flint from Alibates. In most of the intermittent stream valleys that leave the Caprock on the east, there are pebbles of flint that show blue and white, red and white, and often yellow in mottled and banded effects. These pebbles are weathering out of the Ogallala beds of Pliocene age.

Flint that can be visually confused with the type known as Alibates occurs in widely separated localities in west Texas and eastern New Mexico; furthermore, these flints were formed during different geological periods. Therefore it seems that caution should be used in assigning a point of origin to the class of flint identified as Alibates on the basis of visual inspection alone. It is to be hoped that methods will be worked out that will allow definite assignment of flint to known beds and mines. This, of course, would be of great assistance in tracing trade relations, and in determining the popularity and desirability of certain raw materials.

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Texas Technological College
Lubbock, Tex.
February, 1959

A PALEO-INDIAN BISON-KILL IN NORTHWESTERN IOWA

George A. Agogino and W. D. Frankforter

Abstract

The lowest levels of the Simonsen site produced projectile points, scrapers, and knives in association with *Bison occidentalis*. The level is dated by radiocarbon at 6471 B.C. ± 520 years. The triangular, side-notched, and concave-based Simonsen point resembles points from the Turin and Hill sites in Iowa and the Logan Creek site in Nebraska. The close resemblances between the collections from the Simonsen site and the Logan Creek site, which is also radiocarbon dated, provide a basis for dating the essentially similar preceramic material from the Turin and Hill sites.

EXCAVATIONS conducted the past two years at the Simonsen site have produced projectile points (Fig. 1) that may have cultural affiliations with those from several recently discovered preceramic horizons in western Iowa and eastern Nebraska. The Simonsen site, previously called the Quimby Bison-Bed after the nearby town of Quimby, Cherokee County, is located on the Merle Simonsen property, T90N, R41W, Sec. 10, S1/2, NE1/4, along the northwest edge of the Little Sioux River. At this location a layer of bison bones erode from a silty clay horizon of the river bank 15 feet below the top of the alluvial terrace.

Although the presence of these bones was known to local inhabitants for 40 years, their existence only recently came to the attention of archaeologists. In 1956, paleontologist W. D. Frankforter and archaeologist Reynold J. Ruppé investigated this site briefly, but it was not until the summer of 1958 that extensive excavations began under the direction of Frankforter, Sanford Museum, and George A. Agogino, State University of South Dakota. A 12-foot square section was excavated and seven distinct strata recognized, the lowest of which revealed the remains of several partly articulated bison, large fire hearths, crude scrapers, and three broken projectile points. Only one bison skull was in suitable condition for study, and measurements taken on this specimen by Frankforter indicated a variety of animal larger than modern bison and well within the range of *Bison occidentalis*. This suggestion of antiquity was shortly afterward supported by a radiocarbon date of 6471 B.C. (8430 ± 520 years B.P. assigned in February, 1959, by Isotopes Inc., Westwood, N. J.).

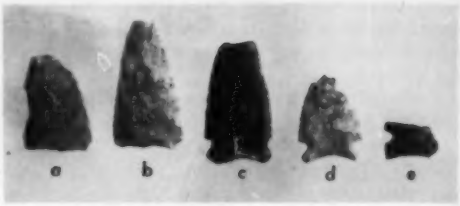

Fig. 1 [Agogino and Frankforter]. Total sample of projectile points recovered from the Simonsen site, Iowa. Length of *b*, 4 cm.

The following season excavations continued with funds supplied by the American Philosophical Society and the *Iowa Des Moines Register*. A volunteer crew, recruited from the Northwest Chapter of the Iowa Archaeological Society and augmented by students from the State University of South Dakota, excavated an area roughly 100 feet by 30 feet. Although work was twice interrupted by flooding of the Little Sioux River, significant additional information was obtained. Three more points and several knives and scrapers were uncovered from the productive horizon. Concentrations of hackberry seed and several wild plum pits were found as were the remains of presently unidentified small birds, amphibians, and several varieties of snail.

Although the remains of more than 25 bison have been uncovered, few good skulls have been excavated. Most specimens are both crushed and weathered, and evidence exists that small boulders were used either to dispatch wounded animals or to split the skull and long bones of dead animals. The heavy concentration of animals in a confined area requires some explanation. Fire seems unlikely as the site lacks sufficient scattered charcoal to suggest a widespread brush fire. Entrapment in a marsh seems ruled out by the fact that the animal long-bones lie parallel to the ancient surface indicating that the bison were on firm ground when dispatched. A sharp incline of 1 inch per foot in the bison horizon occurs to both the north and west of the excavated area. It is possible that future excavations will reveal an ancient ter-

race of sufficient height over which bison might have been driven to destruction.

The Simonsen point is triangular, side-notched and concave-based. The side notches are shallow and exhibit moderate grinding and the base has both thinning and grinding. The points are lens shape in cross section and generally are widest at the shoulder just above the notch. In total length these artifacts range from 25 to 50 mm. The Simonsen points have possible typological affiliations with similar artifacts from three preceramic sites within 100 miles to the south and southwest of Quimby, Iowa. These sites are the Turin and Hill sites in Iowa and the Logan Creek site in Nebraska.

The total point assemblages of the Simonsen and Logan creek sites have close typological agreement. The middle of three cultural horizons at Logan Creek, Burt County, Nebraska, is radiocarbon dated at 4675 B.C. (6633 ± 300 years B.P. assigned February, 1959, by the University of Michigan Radiocarbon Laboratory). The larger points from both the Logan and Simonsen sites bear a strong cultural resemblance to the single projectile point found with the Turin burials, Monona County, Iowa. While the age of these human burials remains controversial, it is interesting to note the geographical position of this site, midway in a direct line between the Logan Creek and Simonsen sites, both of which have produced some Turin-like points. The smaller points at the Simonsen and Logan Creek sites seem typologically similar to points from the Hill site, Mills County, Iowa. The Hill site cultural stratum erodes from a terrace 17 feet below the top of the bank of Pony Creek. Although this site, like the Turin site, has not been radiocarbon dated, it is apparently preceramic and possibly of as great antiquity as either the Simonsen or Logan sites.

A diagnostic side-notched scraper is found at both the Hill and Logan sites that contrasts strongly with the crude side and end scrapers found at the Simonsen site. However, since the Simonsen site is a hunting site, expediency might dictate quickly made, crude scrapers even if the hunters had the ability to create the relatively complex side-notched, and probably hafted, scrapers found at the Logan and Hill campsites.

The present geological picture in this region is of little help to preceramic archaeology as the study of late and post-Wisconsin ecology is far from complete. Detailed mapping has only recently begun. However, with dates already assigned to the Simonsen and Logan sites, and with adequate samples being processed from the Turin and Hill sites, it seems only a matter of time before geological-cultural patterns will be developed regionally for the retreating phases of the last major ice advance and the following anathermal. Certainly the discovery of such seemingly ancient cultural horizons as have been mentioned here indicates a far heavier preceramic occupation in this region than had been suspected in the past. It is hoped that these Early Man discoveries will stimulate further research.

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THE USE OF THE BINOCULAR MICROSCOPE IN THE IDENTIFICATION OF FLINTS

O. F. EVANS

ABSTRACT

The geological study of rocks is oriented toward examining the nature of the rock by means of chemical analyses and by examination with the petrographic microscope. The problem of the archaeologist is one of identification for which the stereoscopic or binocular microscope is best suited. About 20 criteria have been used in identifying with a binocular microscope flints from archaeological sites in Oklahoma as part of a program to trace the sources for the several kinds of flint used in tool making.

FLINT WAS IMPORTANT to primitive men as a material for making chipped stone tools. When a deposit of flint of good quality and considerable size was found it was often worked until large quarries were developed. The products of these quarries, especially the larger and better ones, were often traded over long distances. It is, therefore, important for the archaeologist to be able to identify both artifacts and chips of flint in order to trace these trade relationships. Unfortunately, not all archaeologists are trained in the identification of rocks and minerals. If a student of archaeology does make an effort to obtain adequate training by enrolling in a department of geology, he finds that the rock and mineral courses are given from a different point of view than what he had in mind. The purpose of a geologist in studying a mineral is to determine its kind. In studying rocks he wants to know if two rocks are of the same or different ages. The purpose of the archaeologist, however, is to tell the source of the stone from which artifacts are made. Hence his tools and methods differ somewhat from those of the geologist. Geologists use hand-lens examination and hardness tests in the field, but depend more upon petrographic and chemical analysis in the laboratory for complete identification. Formerly, they made considerable use of the stereoscopic or binocular microscope. The petrographic microscope has so completely replaced the binocular microscope in geological studies, that the value of the later type for the kind of rock and mineral identification needed by archaeologists is generally overlooked. However, the binocular microscope is well adapted to the use of the archaeologist. It is not difficult to use and is small enough to be taken into the field. In the lower powers it gives a clear and relatively broad field of view. For most identification work, magnifying powers of 10 or 12 are most useful, but higher powers are convenient in some cases. A very convenient instrument, therefore, is one having magnification of about 12, 30, and 65 diameters. The method of identification by binocular microscopic examination is relatively easy to learn and use. It gives such definite results that it might well be included as a standard technique in the study of archaeological remains.

In identifying flints from prehistoric sites in Oklahoma, about 20 different characteristics have proven useful as identifying criteria. These include the banding of the material, shape and size of openings, shape and color of inclusions, penetration of light, and kind of fossils. Material from each quarry differs in certain characteristics from the material from other quarries. However, some quarries produce material which exhibits wide variations. Considerable study is usually necessary before one can be sure of his identifications within a large area. Similarly, it is usually not safe to make a determination from a single specimen, although this is sometimes possible. An example of the use of this technique in Oklahoma will illustrate its potentialities. Archaeological studies have shown that there is a cultural change from east to west in Oklahoma at about the 96th meridian. Analysis of flints from these sites by binocular microscopic methods confirms this change. Nearly every important site west of the 96th meridian which has been studied by this method contains material from western sources, the Alibates, Kay County, and Frisco quarries, while sites to the east of this line contain Boone Chert or material from quarries to the east and southeast, but none of the western material.

University of Oklahoma
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June, 1959

RATES OF CULTURE CHANGE IN ARCHAEOLOGY

George I. Quimby

Abstract

Both the rate and the amount of culture change through time may be determined by comparing traits of one temporal segment of a continuum with other segments in progression from the oldest to the youngest segment. Additive and subtractive changes, or innovations and losses, must be evaluated against the time scale rather than balanced against one another, because of the time lag which may occur even if an innovation does produce a corresponding loss.

IN THE SPRING OF 1957, I gave a course at the University of Chicago entitled "Rates of Culture Change in Eastern United States Prehistory." The purpose of this brief paper is to present the method that was used to determine the rates of culture change. Assuming one has command of the data from a cultural continuum such as exists in the Southwest or in parts of eastern North America and assuming that points (the more the better) of this continuum are fixed in "absolute" time the first step is to sort out the cultural changes within whatever temporal segments are to be used. This is most easily done in terms of trait elements, traits, types, or complexes of types depending upon the order of abstraction desired in the results. Let us assume that we are using traits and that the concept of traits has been defined.

By comparing and contrasting traits of one temporal segment of culture with those of other such segments of the same continuum one may find the following: (1) New traits have been added to a temporal segment without loss of traits present in the previous segment; (2) New traits have been added with loss of traits present in previous segments; (3) No new traits have been added but there is loss of traits present in previous temporal segments; (4) No new traits have been added and no traits lost. The rate of change is the total number of new traits and trait losses per unit of time. The unit of time can be the duration of both temporal segments or the elapsed time from the middle of one segment to the middle of the next.

Whichever unit of time is used one should count trait additions or losses in progression from the oldest temporal segments to the youngest temporal segments in the continuum. For instance, the temporal segments, from oldest to youngest in a given continuum might be labeled A, B, C, D, and E. The progressive comparison and contrast of traits in these segments would be in the following order: AB, BC, CD, and DE. This approach measures not only the rate of culture change but the amount of culture change from period to period. It could also be used to measure ratio of culture change by addition to culture change by loss in a given temporal segment or period.

All cultures change by the addition or subtraction of trait elements, traits, types, or complexes. All additive changes are innovations, originating by independent invention or diffusion, and can be described in terms of form, material, use, and technological principles (see Quimby and Spoehr 1951). Any given trait element, trait, type, or complex was at one time an innovation.

All subtractive changes are losses of what once were innovations and they can be described in terms of form, material, use, and technological principles. Any subtractive change can originate within a culture or be the product of diffusion. Although in other contexts cultural losses or subtractions might be considered as innovations they are not considered to be so in this paper. Any innovation or cultural addition may or may not produce a corresponding subtraction. The subtraction, if it does occur may be delayed for a considerable time. Some Upper Great Lakes tribes, for instance, used bows and arrows for a hundred years after they had received flint lock guns, thus there was a hundred year period showing innovation without loss in regard to these particular trait complexes.

There are probably many refinements of this approach to rates of culture change in archaeology. But even with the method as it stands, it is possible to see how much change there was from time to time and how fast the changes occurred. Such information should throw light on processes of culture change and conditions under which it occurred.

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1951 Acculturation and Material Culture — I. *Fieldiana: Anthropology*, Vol. 36, No. 6, pp. 107–47. Chicago Natural History Museum, Chicago.

CHICAGO NATURAL HISTORY MUSEUM
Chicago, Ill.
February, 1959

A DUGOUT CANOE FROM ONTARIO

KENNETH E. KIDD

ABSTRACT

In recent years several dugout canoes have been found in Ontario, but details are lacking for most. One raised from shallow water at Balsam Lake is badly eroded and appears to be of some antiquity. It is a sleek craft of unusual length, remarkably slim proportions and contour, and considerable rigidity, despite its apparent lightness in proportion to its size, because of its substantial bow and stern.

IN AUGUST, 1957, I received word of the finding of a dugout canoe in Balsam Lake, Victoria County, Ontario, and at the first opportunity went to see it. Sgt. Elmer Ewan of the East York police force, who has a summer cottage on the lake, had found it a short time previously and towed it to his beach, where it was allowed to dry slowly. Ewan explained that he had spotted the canoe in shallow water while fishing, and realizing that it might be of some interest and value, raised it and salvaged it. He kindly permitted me to photograph it, to measure it and to make notes. His help and cooperation are deeply appreciated.

When I saw the craft, it was drawn up on the narrow sand beach on the west side of Balsam Lake, more or less in shade and sheltered from winds (Fig. 1). It appeared to be of considerable age to judge from its much eroded condition. Several longitudinal cracks had developed in the bottom, but whether they had come since the beaching was uncertain; the wood was not yet dry and the edges of the cracks seemed as much weathered as the rest. Furthermore, the craft had seemingly flattened somewhat as the fibers lost their strength, both longitudinally and laterally. It was sufficiently well preserved, however, to permit the taking of significant measurements (Fig. 2).

The existing remnant measures 20 feet 3 inches in length and 22¼ inches in width. Due to the breakdown of the gunnels, most of the other measurements of the width as at present observed are not significant. The same could be said for the height of the craft, although at the end these may be of significance: at the stem, the highest point is 10 inches above the keel; and at the stern, 8 inches. It is noteworthy that the highest point of both stem and stern is back from the end of the canoe; that is to say, both ends slant upwards a very slight amount towards the center of the craft. In the case of the stem, the highest point is between 11 and 14 inches from the end point. In the case of the stern, it is 13 inches forward. The thickness of the sides appears to have been in the order of 1½ inches and perhaps of the bottom also, but erosion has been at work and it may have been considerably more than this originally; indeed it seems most likely it was, if sufficient strength were to be imparted to it. There is nothing left to indicate how the craft was hollowed out; no trace of ax marks or of burning survives. That it was done skillfully and with remarkable symmetry and uniformity is still apparent. The entire curve of bow and stern is still visible, and it is worthy of note that the distance between the interior of the stern and its exterior is considerable, 13 inches; in the case of the bow, this interval is 14 inches.

The drawing (Fig. 2) has been accurately prepared from the field measurements and, apart from impon-

FIG. 1 [Kidd]. Dugout canoe from Balsam Lake, Ontario, drawn up on shore.

Fig. 2 [Kidd]. Plan and elevation of dugout canoe from Balsam Lake, Ontario. The drawings are based on accurate measurements, but do not take into account the possibility of warping and slumping. Length, 20 feet, 3 inches.

derables, like the lengthwise slumping due to deterioration of the fibres, must represent a faithful likeness of the craft in its better days. Not every point on the drawing can be substantiated by measurements obtained, but the contours are those obtained, where lacking, by interpolation and are believed to be accurate. Barring idiosyncracies not now detectable in the remains, they must be so. Close observation will reveal first, the great length of the craft; second, its remarkably slim proportions; third, its slim contours and fourth, its apparent lightness in proportion to its size, yet the rigidity provided by the substantial bow and stern.

Unfortunately the Museum was not in a position to acquire the canoe for its collection, and its present whereabouts are not known. Nor was it possible to obtain a radiocarbon date. The shape of the craft and its eroded condition suggest, however, some antiquity. There are several other dugouts, mostly fragmentary, in local museums in Ontario but they are not the sleek craft which this appears to have been. There are or were four in the Bruce County Museum and one or two in the Niagara Historical Society Museum. In the past few years, four others have come to my notice, all of them from the Shield country; those of which photographs were seen appeared not to have been old. One still remains to be investigated. Another is said to have been found near Lake Ignace in 1936 and from a press photograph appears to have some resemblance to the one described here, although details are lacking and the specimen is apparently lost (*Fort William Times Journal*, April 11, 1958). In view of the increasing number of finds of dugouts, it seems worthwhile to put on record the details of this rather impressive specimen for the purpose of comparison.

Royal Ontario Museum
Toronto, Canada
February, 1959

PHOENIX BUTTONS

Emory Strong

Abstract

Metal buttons bearing the Phoenix bird, a motto in French, and a number are found in quantity in historic sites along the lower Columbia River, and less commonly throughout western North America. Phoenix buttons were not made for Napoleon, as often claimed, but were manufactured by an English firm in the early 1800's for King Christophe of Haiti. The Phoenix bird and the motto come from his coat of arms; the numbers refer to regiments. These military uniform buttons were brought to the Northwest before 1835 by an independent trader, most likely Nathaniel Wyeth, who probably used uniform coats, which he may have obtained earlier when shipping ice to the West Indies, to trade for fish for his salmon packing plant on the Columbia River.

ONE OF THE MOST interesting objects of historical archaeology is the Phoenix button, found in large numbers in the lower Columbia River valley and in much lesser quantities along the western coast and back into the plains. They have been excavated in mission sites in California and old village sites in the Southwest, one even in Mexico; they seem to have attained nearly nation wide distribution, although in small numbers. Specimens are in most of the large museums and are treasured in the show cases of many button collectors. But in spite of their extensive distribution their origin and history have never been completely traced.

The Phoenix button is found in three types, discs 0.95 and 0.60 inch in diameter, and a ball 0.65 inch in diameter. Each button bears a replica of the legendary Phoenix bird, the motto "Je Renais De Mes Cendres," and a number. The disc buttons are found in four different styles which I call styles 1, 2, 3, and 4 (Fig. 1). On each the motto is the same but the bird is different. The numbers, which are regiment numbers, run from 1 through 10, 14, 20, and 25 through 30 in the disc type, and 1 and 2 in the ball. Some numbers are quite rare; I know of only one number 3 large, numbers 10 and 14 large are quite rare but number 14 small is one of the most plentiful. Ball type buttons are very rare; I have seen only four but know that others have been found. The disc type is made of both brass and bronze, the ball type is copper and was once silver plated.

Style 1 (Fig. 1 *a*) large is found in all the known numbers except 3, 29, and 30; the small in all except 2 and 29. Style 2 (Fig. 1 *b*) occurs in numbers 27 and 29 large, and 29 small. Style 3 bird (Fig. 1 *c*) is on numbers 3, 6, 7, 25, and 30 large and 7 and 27 small. The style 4 (Fig. 1 *d*), with the cannons, is found only in numbers 1 and 2, but there are three different styles of birds, matching styles 1, 2, and 3. This tabulation I have made from an examination of all the collections I have been able to locate, amounting to possibly 400 buttons, and my own collection of 80 which includes all the numbers except 10. The tabulation may not, therefore, be complete, but if there are any other numbers they are extremely rare.

None of the disc buttons have any markings on the back, but the ball type bears the inscription "Bushby-London" encircling the shank. The Registrar of Business Names in England has no record of this firm. Since the lettering on the ball and disc types is the same style and size, it seems reasonable to assume that they were all made by the same English firm and if so it eliminates the possibility that they were made for Napoleon, a theory that is sometimes advanced (MacLennan 1953:

FIG. 1 [Strong]. Large and small disc types of the four styles of Phoenix buttons. *a*, style 1; *b*, style 2; *c*, style 3; *d*, style 4.

21). Furthermore, the complete catalogue of French military buttons compiled by Louis Fallou illustrates no Phoenix birds.

It has been established that the buttons were made for King Christophe of Haiti but whether they were ever delivered to him is unknown. Christophe used the Phoenix bird extensively. An old cut shows it on his crown and what appears to be a coat of arms, and it is on his officers' sword belt buckles complete with the same motto. They arrived on the Columbia River prior to 1835 because they are found in the old Multnomah village site of the Chinook Indians on Sauvies Island near Portland, Oregon, a site known to have been deserted by that date. It is known that the Hudson's Bay Company did not bring them in, and their concentrated distribution on the lower river seems to eliminate the North West Company which did extensive trading on the upper river. Because they are so abundant in the Cowlitz–Willamette–Cascades part of the Columbia this must be considered their original center of distribution. The buttons are comparatively rare on the middle Columbia only a hundred miles or so above Portland.

The late Norma G. Seaman, student of Oregon history, and Mrs. Nelly Church, button collector, after an extensive correspondence with various museums and button authorities, reached the conclusion that the buttons were originally part of the trade goods of Nathaniel Wyeth who came West in 1832 and again in 1833, intending to start a salmon packing and trading venture on the Columbia (Wyeth 1905). Prior to coming to Oregon, Wyeth shipped ice to the West Indies and could have obtained the buttons there, as he was already planning his Oregon adventure. It is recorded that he had buttons among the trinkets he used to purchase salmon. Phoenix buttons are found in the largest quantities in the favorite fishing places near his Fort William on Sauvies Island; the Cowlitz and Clackamas rivers, the falls at Oregon City, and the Cascades. These places were about as far from his packing plant as salmon could be transported in canoes without spoiling. Several hundred buttons have been found near these places; at one time 200 were found in one cache, and 26 were picked up from the clay on the river bank on Sauvies Island. They can be found on every one of the vault burial sites on the lower river; in the early days burial was by exposing in sheds or in elevated canoes on rocky points; all has of course disappeared long ago except the metal and stone. Since this area is about 100 miles from the ocean the coastal trading vessels must be eliminated as a source of the buttons.

We know that Phoenix buttons were made for Henri Christophe by an English firm in the early 1800's and were brought to the Columbia prior to 1835. Who brought them here is not known, but it was one of the independent traders and the evidence points strongly to Wyeth. Since but a small percentage of the buttons are perforated for suspension they probably were sold attached to uniform coats, one of the favorite early trade items especially if highly colored. Occasionally, a few are yet found clinging to fabric that was preserved by their copper salts.

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4704 ST. JOHNS ROAD
Vancouver, Wash.
March, 1959

THE RAPE OF WAYLAND

D. S. Byers

Abstract

The removal of loam from the grounds of the Old Mansion Inn, Wayland, Massachusetts, exposed blackened areas which were investigated by local collectors. The site was obviously a rich and unusual burial area, probably including cremations, with good possibilities for radiocarbon dating of the entire tool assemblage. Unfortunately, the site was destroyed within a very short time by children, their parents, and friends, assisted by some local collectors. Many members of the Massachusetts Archaeological Society were shocked that the collectors who participated in the looting of the site rather than protecting it for serious study were members of the Society. This incident points up the need for state and local societies to discover some way of disciplining members who do not conform to accepted standards of archaeological work.

ONE OF THE MOST TRAGIC events in the history of northeastern archaeology occurred in June, 1959. Real estate developers removed loam from the grounds of the Old Mansion Inn in Wayland, Massachusetts and in doing so exposed blackened areas rich in charcoal. Two collectors who live in the neighborhood took note of the black areas and commenced to investigate. Within a short time it became apparent that they had uncovered something out of the ordinary, yet they continued on their own without requesting institutional assistance. They found a number of caches of large but extremely thin corner-removed blades; 174 of them were in one cache. Much charcoal blackened the soil; the collectors spoke of several fragments of burned bone. Some of the bone may have been human bone, some may have been animal bone. I saw at least one deciduous human incisor.

Objects recovered, in addition to the large thin blades, include fragments of pestles, grooved axes, and what may have been gouges and adzes. One man picked up a copper adz or gouge; it bore the impressions of what may have been cording or a textile fabric. Fragments account for at least one large, shallow, soapstone bowl. A lug handle was at each end of this vessel. Other sherds suggest the possibility that additional soapstone vessels were in the loot taken from the site.

Children learned of the discovery, and the news spread like wildfire. The press, radio, and even television carried accounts of the discovery. Publicity brought other collectors from Maine, Connecticut, New York, and New Jersey. Local children, their parents, and friends, with assistance from some local collectors destroyed the site before outsiders arrived.

It is difficult to say for certain just what was involved. Quite possibly a number of pits contained cremated burials; just how many will never be known. Nor will there be any evidence regarding the relationship of graves to one another or to larger pits. At the bottom of the rooted-over area were remains of two graves; one had been dug down through the other. Red ocher is said to have been found in the graves. Heavily pigmented areas which probably resulted from natural weathering of the soil were later found in areas outside the graves.

The R. S. Peabody Foundation was called by one of the founders of the Massachusetts Archaeological Society, shocked at the turn of events. The Foundation, with the consent of the owner, endeavored to protect the site until adequate steps could be taken to ensure salvage of remaining scraps of evidence. In this it was not successful.

It is tragic to learn that the discoverers of the site were card-holding members of the Massachusetts Archaeological Society. It is startling to learn that some members of that society used their cards to misrepresent to the watchmen their relationship to the Foundation. It is shocking to discover that some members of that society used their membership cards to strike awe in the minds of little children in order to induce them to relinquish relics.

It is obvious that state societies will have to discover some way of disciplining members who do not conform to ethical standards for archaeological work. At least one of the discoverers of the Mansion Inn site had assisted at a dig run by professionals before the war and had been Chairman of one of the Chapters of the Society. Both men knew what they should have done. It is encouraging to learn that many members of the Massachusetts Archaeological Society are shocked and dismayed at the lootings. In this attitude lies hope for survival of this group as an archaeological organization. These members should be given every assistance by all true archaeologists.

With the looting of the Mansion Inn site has gone not only all chance of learning about a most unusual burial site, but also chances of obtaining radiocarbon dates for the occurrence of a whole array of tools. Quite possibly we shall never have a like opportunity again.

R. S. Peabody Foundation
Andover, Mass.
October, 1959

OBSERVATIONS ON THE BUTCHERING TECHNIQUE AT A PREHISTORIC BISON-KILL IN MONTANA

Thomas F. Kehoe and Alice B. Kehoe

Abstract

The analysis of the bone from a bison drive in north-central Montana corroborates the implications of White's analyses of bison bone from Plains villages: the heaviest non-meaty bone was left at the kill site, the bones rich in marrow were chopped up for marrow-extraction; even when many animals were available all were thoroughly utilized by the hunters. This excavation also confirms ethnographic data on season of drives, methods of butchering, and use of arrows for killing.

DURING THE SUMMER OF 1958 the senior author excavated the bone midden of a bison drive site, 24GL302, on the Blackfeet Indian Reservation, Montana. The site, which had been tested in 1952, consists of a stream-cut bluff and the terrace between the bluff and the stream. This terrace, partially enclosed by a curve in the stream, supports thick brush. It forms a natural corral in which bison stampeded over the bluff could be slaughtered. It is likely that the hunters further prepared the area by burning off the brush below the bluff and building a log fence around the periphery of the burned sector.

Two layers of compacted, disarticulated bison bones were discovered beneath the bluff, and each layer is believed to represent the remains of a prehistoric bison drive. In addition, the skull, atlas, and other fragments of a young bison bull were observed in association with a projectile point 8 feet below the surface just at the base of the slope. No other animals were discovered at this depth, several feet below the lower drive layer. Mixed in the two drive layers were arrowpoints of Late Prehistoric Plains types, stone knives, crude heavy choppers probably made from local river cobbles, and a few bone awls and worked fragments of bone that may have been used as marrow-scrapers.

In the analysis of the bone material from 24GL302, every effort was made to obtain data that would be strictly comparable to the tables published by White (1953, 1954, 1955, 1956) on bison remains from village sites in the Plains. The bison bones were individually compared to bison skeletons donated to the Museum of the Plains Indian from the herd on the National Bison Range (Moiese, Montana) by the Manager, C. J. Henry. No significant differences were observed between the recent and the prehistoric bison, nor were any pathological conditions observed in the latter. Non-bison bones, which formed only a very small percentage of the recovered bone, were checked by Robert Scriver of Browning, an experienced taxidermist. With the exception of a portion of unworked elk antler, these non-bison bones appeared to belong to rodents and coyotes, and were probably not introduced into the site by man.

Both drives at 24GL302 probably took place in late summer or early autumn, for no fetal or newly-born calves were discovered, and the proportion of mature bulls to cows was such as should occur only during or near the bison mating season. According to both historical and ethnographic accounts (Wissler 1910: 41), autumn was the favored season for hunting bison, and of course for preparing winter stores of dried meat and pemmican. Site 24GL302 would be especially suitable for pemmican preparation, since even now it produces quantities of sarvisberries and chokecherries, the choice pemmican flavoring. Wissler (1910: 21) mentions that in the ripening season, late summer, Blackfoot camps were usually pitched near favorable berrying areas.

The bison killed in the drives — an undertaking that apparently required an average of three or four arrows per bison — were butchered where they had fallen. From the ratio of knives to bison, each stone knife must have been used on at least two or three animals. The frequencies of the various skeletal elements (Table 1) at 24GL302 indicate these bison had been "heavily" butchered, in the Blackfoot term (Wissler 1910: 42), that is, cut up so that the maximum quantity of meat could be taken. Since bison were not heavily butchered unless the meat could be processed without lengthy transportation from the kill site, it is likely that the meat was pre-

TABLE 1

FREQUENCY DISTRIBUTION OF BISON BONES, 24GL302

Element	*No. of Individuals**	*Per cent of Greatest No.*
Skull, occipital portion	16	43.2
Skull, maxilla only	8	21.6
Mandible	37	100
Vertebra, atlas	20	54
Vertebra, axis	16	43.2
Vertebra, cervical	15	40.5
Vertebra, thoracic	12	32.4
Vertebra, lumbar	6	16.2
Vertebra, sacrum	8	21.6
Vertebra, caudal	4	10.8
Scapula (mostly spines only)	24	64.8
Humerus, proximal	5	13.5
Humerus, distal	7	18.9
Radius, proximal	6	16.2
Radius, distal	13	35.1
Ulna, proximal	10	27
Ulna, distal	3	8.1
Carpals: Cuneiform	6	16.2
Magnum	21	56.7
Lunate	8	21.6
Scaphoid	11	29.7
Unciform	7	18.9
Metacarpal, proximal	18	48.6
Metacarpal, distal	17	45.9
Pelvis, acetabulum	12	32.4
Pelvis, ilium	4	10.8
Pelvis, ischium	1	2.7
Pelvis, pubis	1	2.7
Femur, head	6	16.2
Femur, head and proximal only	3	8.1
Femur, proximal, no head	2	5.4
Femur, distal	6	16.2
Femur, proximal half	6	16.2
Patella	6	16.2
Tibia, proximal	6	16.2
Tibia, distal	14	37.8
Astragulus	19	51.3
Calcaneum	18	48.6
Naviculo-cuboid	12	32.4
Cuneiform, pes	4	10.8
Metatarsal, proximal	11	29.7
Metatarsal, distal	13	35.1
Phalanx, first	14	37.8
Phalanx, second	18	48.6
Phalanx, third	14	37.8

* Minimum

TABLE 2

Percentages of Bison Bones, Northern Plains Sites

Element	24GL302 Mature (late prehist.)	24GL302 Total (late prehist.)	32ML39 * Night-walker's Butte	39ST30 — Dodd Site * Monroe pre-1500 A.D.	39ST30 — Dodd Site * Anderson pre-1500 A.D.	39ST30 — Dodd Site * Stanley 1700–50	39ST? * Phillips Ranch 1750–1800	39ST6 * Buffalo Pasture 1750–1800?	32ME15 * Rock Village 1825–37
Skull, occiput	64	43.2	7.3	10	5.5	21	2.5	0	15.8
Skull, horn cores	18	13.5	—	12	2.8	10.5	16.6	36	15.6
Skull, maxilla	28	21.6	43.7	20	41.6	42.1	6.4	18	37
Mandible	100	100	100	93.3	80	100	24	60	96
Vertebra, atlas	44	54	14.5	10	14	5.2	9	4.3	15
Vertebra, axis	32	43.2	9	0	2.8	0	2.5	3.6	11
Vertebra, cervical	24	40.5	3.7	8	11	5.2	5	1.4	6
Vertebra, thoracic	20	32.4	3.7	5	5.5	5.2	2.5	1.4	7
Vertebra, lumbar	12	16.2	1.8	6	8.3	5.2	1.2	0	8
Vertebra, sacrum	12	21.6	0	0	8.3	0	2.5	1.4	8
Vertebra, caudal	16	10.8	0	3.4	2.8	5.2	1.2	3.6	0
Scapula	92	64.8	70	85	92	26.3	100	100	45.7
Humerus, proximal	16	13.5	30	20	8.3	26.3	20	24.6	8.5
Humerus, distal	20	18.9	40	45	52	47.3	25	26	77
Radius, proximal	16	16.2	27.7	62.6	61	15.7	20	21.7	46
Radius, distal	24	35.1	17	40	30	31.5	14	14.5	44
Ulna, proximal	16	27.0	21	60	52	21	11.6	15	40
Metacarpal, proximal	52	48.6	20	28.8	30	15.7	15	14.5	22.5
Pelvis	40	32.4	19	18	23	31.5	5	4.3	7
Femur, head	16	16.2	10	17	25	15.7	7.7	8	22
Femur, proximal	20	16.2	1.8	7	14	10.5	3.8	6	28.6
Femur, distal	12	16.2	17	6	12.5	5.2	—	—	18
Femur, complete	4	2.7	—	—	—	—	7.7	6.4	—
Tibia, proximal	20	16.2	3.7	8	14	5.2	10	8.7	30
Tibia, distal	32	37.8	54.5	100	94	63.1	30	31	100
Astragulus	76	51.3	10	52	100	47.3	12	22.4	18
Metatarsal, proximal	40	29.7	3.7	26	20	15.7	16.6	10	—
Metatarsal, complete	28	21.6	—	—	—	—	—	—	20

* All data in these columns from White 1953, 1954, and 1955.

pared at or near the site. Only the blade of the scapula appears to have been cut out for tool-making; other bones were either left intact or crudely broken, obviously during butchering or for marrow extraction.

Once the animals had been skinned, the hunters must have quartered the bison. This was done by either cutting through the scapula-humerus joint or cutting off the scapula with the foreleg, and by chopping through the pelvis so that the acetabulum remained with the hindleg. The backbone was chopped through the lumbar region or through the sacrum, perhaps to facilitate removal of the prized back-fat. The meat was stripped off the ribs and "boss-ribs" (thoracic spines). The skull was turned upside-down and the mandibles and tongue removed, either by smashing the glenoid fossae or by chopping through the ascending rami of the mandibles. The brains were removed by smashing the basal section of the cranium; the facial section was also often destroyed, presumably to obtain the nasal cartilages. Each limb was cut apart at the joints or, more rarely, through the shafts of the bones, and the meat removed. Finally, the humeri, radii, ulnae, femora, and tibiae were split and chopped up, usually with a sharp-edged heavy instrument (such as the halved cobbles frequent at the site), and the marrow extracted either by scraping or by boiling bone fragments reduced to less than four inches in length. Few podial bones were found broken, but on the other hand magna outnumber the other carpals, and astraguli and calcanea the other tarsals, suggesting that the proximal end of the wrist joint and the distal end of the ankle may have been chopped through. Marrow-containing bones and the bones inferred to have been chopped in butchering occurred in significantly low percentages, while the other skeletal elements, with the exceptions of the most fragile small bones (hyoid, caudal vertebrae, carpals, patellae) and of mandibles, occurred in approximately the same frequencies. Mandibles represented the greatest number of individuals, probably because many mandibles were discovered placed in small piles (centers at which the aborigines collected the tongues?), and the excavation trench thus chanced to recover more.

Table 2 compares the percentages of the various skeletal elements recovered from 24GL302 to the frequencies of these elements at several Plains village sites, dated

from pre-A.D. 1500 (White 1953: 160) to 1837 (White 1954: 260). It thus compares the butchering of bison at a kill site near a nomadic camp to the utilization of bison killed outside sedentary villages. The table confirms the supposition that the heaviest non-meaty bones (skull, cervical vertebrae, metapodials, pelves, podials) were left at the kill site if bison meat had to be transported. Differences in the frequencies of other bones, particularly vertebrae, pelves, and limbs, appear to reflect variations in cultural habits of butchering and food preferences, while scapulae and portions of the skull may indicate by their frequencies whether or not they were desired for tool materials. Thus is should be evident that tables of the frequencies of the various skeletal elements of large food animals at archaeological sites can both distinguish between cultural groups as shown by White, and between the camps of hunters sent out from villages and the kill sites and camps of nomadic hunters.

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WISSLER, CLARK

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SASKATCHEWAN MUSEUM OF NATURAL HISTORY

Regina, Sask.

June, 1959

ANNOUNCEMENT OF THE ANNUAL MEETING

The 25th Annual Meeting of the Society for American Archaeology will be held on May 5, 6, and 7, 1960, in New Haven, Connecticut, with the Department of Anthropology, Yale University, as host. The Hotel Taft will serve as headquarters for the meeting. Formal sessions will be held partially in the hotel and partially in Yale buildings.

To celebrate the 25th anniversary of the founding of the Society, a symposium is planned at which leading specialists will summarize the principal intellectual developments in the archaeology of their respective areas over the past 25 years. Other symposia will deal with ties between the archaeology of the arctic and the temperate zones of North America and with the teaching of archaeology in our universities. There will also be a number of general sessions.

Irving Rouse is Program Chairman and James A. Bennyhoff, Chairman of the Local Arrangements Committee. Send titles of papers to be presented at the general sessions and brief abstracts (100 to 200 words) before March 1, 1960, to Rouse at the Department of Anthropology, Box 2114, Yale Station, New Haven, Connecticut. Reservations for rooms should be made directly with the Hotel Taft, 265 College Street, New Haven, Connecticut.

REVIEWS

EDITED BY DONALD COLLIER

The Origin of Civilized Societies. RUSHTON COULBORN. Princeton University Press, Princeton, 1959. x+200 pp. $4.00.

This volume essays to be a systematic, comparative treatment of the revolutionary episodes by which civilized life independently came into being in Egypt, Mesopotamia, Crete, the Indus Valley, North China, Middle America, and the Central Andes. Its author, a historian at the University of Atlanta, is familiar with not a little of the specialized viewpoint and data of archaeology and has been concerned for many years with comparative institutional history. Unfortunately, the high promise of his enterprise is not realized; the book, in fact, offers not a new synthesis but a contrived defense of a curiously narrow thesis.

Briefly put, the burden of Coulborn's argument is that the onset of severe dessication after the end of the Pleistocene began "a simple chain of cause and effect." He maintains that it first drove primitive desert dwellers to the invention of agriculture as a sort of "desperate experiment" for survival in the increasingly restricted zones which remained habitable, a view of agricultural origins that finds little support in the distribution of pertinent archaeological remains of potentially cultivable species. Continued dessication, coupled wtih "congestion" resulting from population increase and the exhaustion of arable lands, then led them to migrate as groups, under conditions of great stress and agitation. In the Old World and Peru, groups moving down into "wild, swamp-filled" or otherwise forbidding valleys generally learned the arts of irrigation and water control during a sojourn on the valley margins or upper tributaries. In Middle America, this was supposedly paralleled by groups driven southward from the Southwest by dessication until they finally took refuge in the uninhabited lowlands where water was plentiful but "soil was obtained only at the price of the terrible and almost continuous burden of clearing the tropical forest." In short, he believes that "the combination of possibilities with tasks, of opportunities with obstacles, is the largest aspect of the common material conditions at the origin of all the primary civilized societies." The intellectual debt here to Toynbee's notions of "challenge and response" is not acknowledged, perhaps because it is so obvious.

The varieties of Coulborn's seven civilized societies nowhere are even briefly described in terms of the empirical findings of archaeologists or historians, nor is what he means by civilization ever very clearly defined. General criteria like writing, urbanism, and greater over-all complexity are summarily disposed of as "easy answers" not having to do with the substance of civilization; "it is a mistaking of result for cause, and indeed of only one kind of result among many." Following Sorokin and Kroeber, he decides that the only unambiguous distinctions between primitive and civilized societies lie in the pattern of development and the aesthetic aspect of the latter. "If a society is found to develop rapidly and to move historically through cycles of rise and fall, then the society is civilized. Any society, civilized or primitive, is to be identified by its style."

If we are left in some doubt as to what civilization is, the author at least is explicit on how he believes it came about. The sequence of changes having been initiated and maintained by stringent environmental pressures, the subsequent rapid transformation from primitive to civilized is stated to have been a consequence of the rise of new religions with charismatic leaders. "Religion was the positive, the human-mental, agency of the creation of the primary civilized societies, and it has been so in the creation, or re-creation, of all civilized societies." Using a free-wheeling mixture of "conglomerate" origin myths, traces in the archaeological record, and "analogies" drawn from modern nativistic movements, he concludes that the really effective new belief in all cases was water worship, "a great new gospel" which alone provided "the special answer to the question of the hour."

Space does not permit a point-for-point dissection of the reasoning and evidence that is advanced in support of this argument. It frequently seems to depend on an incomplete or even capricious selection of sources that overlooks or ignores bodies of evidence tending to controvert his thesis. His most basic postulate of severe post-Pleistocene dessication in southwestern Asia, for example, is contrary to the picture of moderate and fluctuating change which most recent authorities would draw. Similarly, the case for primacy of water worship relies largely on a distortion of the role of water gods, as with Enki in Sumer, or an over emphasis of the specifically aqueous aspects of deities more broadly identified with regeneration and growth, as particularly with Osiris.

Other views, confidently put forward, are even more questionable. Mesoamericanists will note with surprise the categorical statement that the Mayas formed "the leading part" of Mesoamerican society, and that (presumably in the Preclassic as well as later) the "Guatemala highlands were, clearly, provincial territory, following the adjacent lowland territory of central Yucatan in development." They also may be puzzled at the clearly arbitrary judgement which places their area in a "civilized" category already in *pre*-"Archaic or Middle Culture" times, a level of development which elsewhere Coulborn writes was not attained in the Andean area until the Chavin horizon. Specialists familiar with recent work in Mesopotamian and Egyptian prehistory will be equally puzzled to learn that during the last decade opinion "has crystallized firmly" in favor of an original occupation of Egypt by agriculturalists moving northward from Abyssinia, and that the earliest settlers in Mesopotamia "had come from Iran."

Many of the migrations that Coulborn sees as an essential stage in the emergence of civilized societies, it may be

observed, are posited on a mixed basis of dubious fact and gratuitous assumption. Why, for example, should agriculture have been developed in the Middle American uplands by "refugees" (for whom the reviewer knows no evidence) from increasingly arid areas to the north, rather than by the population already in residence there? On what conceivable basis should the "main human population" of the New World be viewed as the product of "great mesolithic migrations" originating in the zone of supposedly increasing aridity in western Asia? Few anthropologists will find the casual citation of references to photographs of New World Indian physical types adequate — or even pertinent — for this purpose.

In summary, the volume fails to carry conviction for many of its specific reconstructions as well as for its general thesis. However, it does deserve commendation as one of the tiny handful of genuinely comparative efforts in our field, and we may hope at least that it contributes to a renewal of interest in this highly worthwhile end. Moreover, Coulborn rightly has chosen to stress the importance of ideational factors which archaeologists are too prone to ignore. So long as temple plans and figurines are merely considered as isolated cultural features without reference to the body of religious understanding and ritual they symbolize, an essential component in the origin of civilizations may continue to escape us.

ROBERT M. ADAMS
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Chicago, Ill.

The Textile Arts. A Handbook of Fabric Structure and Design Processes: Ancient and Modern Weaving, Braiding, Printing, and Other Textile Techniques. VERLA BIRRELL. Harper and Brothers, New York, 1959. xvi + 514 pp., 217 figs., 7 pls. $12.50.

In her preface to this wide-ranging publication on the textile arts, the author states that "this book considers all phases of fabric formation and decoration" and goes on to enumerate the "variety of interests" whose needs it "has been compiled to serve": the student of textile design ("complete instructions are given for each of the various textile design processes"), the archaeologist ("textile techniques are also carefully analyzed and classified"), as well as the weaver, the recreational director, the art teacher, the home economist, the homemaker, the home craftsman, "and those interested in various hobbies."

A finite book, even one of 514 pages 7 by 10 inches in size, could hardly be expected to serve all these interests either adequately or impartially. The present reviewers are chiefly concerned with the extent to which "the archaeologist will," as Birrell states in her preface, "find this book a standard general reference, a reference text for classes in primitive technologies, and a help with problems of textile identification."

As a reference text for classes in primitive technologies the book has its drawbacks. The author displays a rather hazy grasp of the terminology and facts of archaeology in such statements as: "LeCoq discovered a fragment of this type of knotted-pile in Turkestan in the ruins of a fifth or sixth century culture strata" (p. 176); and "In North America the weaving arts were not too well developed. . . . For the most part their textiles were simple, but some tapestry weaving has been found in the Southwest" (p. 16).

Although it must be conceded that Birrell is adroit in compiling an incredible amount of detail from a great variety of sources (including her own experience), the result is a disconcerting potpourri of fact, folklore, and schoolroom instruction.

The archaeologist seeking to use the book for purposes of textile identification will find himself handicapped first by the fact that various techniques are classified and discussed from different points of view, so that the reader must constantly refer back and forth from one part of the book to another for information pertaining to a single fabric type (often without the aid of specific page references). For example, the major discussion of tapestry is in the chapter entitled "Rug Looms and Rug-Making Techniques"; it is also discussed to some extent in "Belt Looms and Belt Weaves," and very briefly in "Basic Weaves" (although Birrell states elsewhere that "The tapestry weave is usually considered a variation of the plain weave" [p. 162]). Furthermore, the information given in one section sometimes conflicts with that given in another.

The archaeologist may find that his problems of identification are further complicated by apparently haphazard illustration (and captioning). In one of several sections of the book in which *twining* is discussed, although 39 types and subtypes are listed consecutively under three main headings and five subdivisions, only 14 are illustrated diagrammatically. The 14 illustrations are all of *weft*-twining, selected apparently at random; whereas none of the types of "*warp*-twining and related techniques" is diagrammed. Nor are the relationships and distinctions between "warp-twining" and "tablet weaving" ever made clear.

The archaeologist might well feel that identification of his specimens would entail a rather excessive amount of practical experimentation in the numerous instances in which the only clue to the nature of a type of fabric structure is to be found in directions for making it — whether the directions are step by step accounts of procedure (for example, bobbin lace, p. 335) or "drafts" of threading and loom tie-up supplemented by directions for treadling (for example, "Indian Saddle Blanket Weave," pp. 266–7).

The *Glossary* listed in the Table of Contents proves to be only the most extensive of several included in the book. Some terms are differently defined in different glossaries, while other important technical terms, such as twining, used in the text are not defined in any. A number of familiar terms are given special — unfamiliar — meanings, and some new terms provided for familiar technical variations (often by the addition of the prefix "pseudo-"). In general, there is a singular lack of con-

sistency in the terminology of text, figure captions, index, and glossaries; and an unfortunate number of typographical errors and misspelled words adds to the confusion.

The interworking of various systems of headings and subheadings with lists of types, subtypes, and variations suggests a much higher degree of organization of the whole work than is borne out by more careful study; and perhaps of all those whose interests Birrell seeks to serve, it is "those interested in various hobbies" who will be most beguiled by the illustrations and historical tidbits and most inclined to try to discover if indeed "Bobbin lace is made as follows: . . . ." (p. 335).

Irene Emery
Mary Elizabeth King
The Textile Museum
Washington, D.C.

The Elements of Style. William Strunk, Jr., with Revisions, an Introduction, and a New Chapter on Writing by E. B. White. Macmillan, New York, 1959. xvi+71 pp. $2.50.

This is a revision of the "little book" which Professor Strunk used to assign his classes in English 8 at Cornell 40 years ago. Although it will not take the place of standard manuals of style, it should be carefully read and studied by anyone who attempts to write a readable monograph or paper.

The book comprises seven divisions — A Note on This Book; Introduction; I, Elementary Rules of Usage; II, Elementary Principles of Composition; III, A Few Matters of Form; IV, Words and Expressions Commonly Misused; V, An Approach to Style (With a List of Reminders). Chapters I and II cover 18 simple rules. Random selections include: 5. *Do not join independent clauses by a comma;* 7. *A participial phrase at the beginning of a sentence must refer to the grammatical subject;* 10. *Use the active voice;* 13. *Omit needless words.* Titles of the other chapters are self-explanatory.

American archaeology can present an absorbing story to that mythical being, the intelligent layman. The literature of American archaeology, on the other hand, gives overwhelming evidence of what appears to be a concerted effort to kill that interest. Strunk and White seek to prevent such unnecessary slaughter. As White says in his introduction: "Will [Strunk] felt that the reader was in serious trouble most of the time, a man floundering in a swamp, and that it was the duty of anyone attempting to write English to drain this swamp quickly and get his man up on dry ground, or at least throw him a rope."

This book will not make a writer of every contributor to American Antiquity. Read and digest it and you may yet spare a laboring Editor hours of agony spent in rendering into English that which should be English, but which is not because the author failed to observe simple rules. [Editor's note — Also to be recommended is "The Writing of American Archaeology," by Margaret W. Harrison, published in American Antiquity, Vol. 10, No. 4, pp. 331–9, April, 1945, when the reviewer was Editor of this journal.]

You cannot expect to become a Thurber or a White overnight, but you can hope to make yourself understood by people who never heard of your subject before. Your editor, and the often unrecognized assistant who may have had the misfortune to marry him, will be forever grateful.

Douglas S. Byers
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Tebiwa. Edited by Earl H. Swanson. Vol. 2, No. 1, Winter, 1958–59. Idaho State College, Idaho, 1959. 86 pp. $1.50.

The Idaho State College Museum has presented us with what may be considered as a new journal. Under the name *Tebiwa,* the journal of the Idaho State College Museum re-appears in an enlarged format with a reduction to two numbers per year. This name is an adaptation of a Bannock word for "homeland" or "native land." The editor states that ". . . Under this definition we expect to publish papers from a variety of disciplines, with area emphasis on the Northwest. This will not thereby exclude papers of wider interest, nor is it intended that they only be products of original research; (some) polemics are welcome." Fields intended for treatment include paleontology, zoology, geology, and history, as well as archaeology and ethnology.

The first issue (Winter, 1958–59) carries six papers dealing with Northwest archaeology in addition to such "housekeeping" sections as "Notes and News" and "Museum accessions." There is a certain unity in these papers in that they are all the products of former students of the University of Washington and the editor pays tribute to Douglas Osborne's influence there.

Donald R. Tuohy reports the archaeological surveys in connection with several pipeline installations and properly recognizes that in an area with the archaeological diversity of the Columbia Plateau, and given the inadequacy of our knowledge of the area, it is of value to know the areas of little occupancy. This is often not reported for it is sometimes considered to be a thankless task.

B. Robert Butler discusses the archaeological occurrence of implements which have ethnographically been associated with the dice game. None of these stick dice have great time depth in the Dalles area, and there are certain typological differences in the implements as they progress through time. Butler proposes that the game is of relatively recent introduction into the area. Findings at other sites in the Southern Plateau bear this out. He indicates the Southwest as a possible point of origin.

Earl H. Swanson, who edits the journal, has included a survey of the Methow Valley in north-central Washington. Both circular and rectangular housepits were discovered. Talus pits were encountered, which, if correctly identified as ". . . a modified form of earth oven," pre-

sents us, to the best of my knowledge, with a unique occurence in the Plateau.

Claude N. Warren, Donald R. Tuohy, and Alan L. Bryan present reports of surveys, and based upon these, appraisals of southwestern Washington archaeology. Running through each of these is the problem of the interrelationships of the Coast and the Plateau areas. Bryan proposes a "Trans-Cascadian" province which is transitional from the maritime to the desert orientation.

All of the papers are seriously handicapped by the lack of maps. It is promised that maps will be included in future issues, which will be printed by letterpress. The papers are rather uneven in treatment and some inconsistencies may be noted. Rectangular housepits and houses in single line parallel to the river are treated by Warren as Coastal traits although Swanson reports rectangular housepits from the Plateau in an article in this same issue. They have also been reported earlier. As for houses arranged in lines parallel with the river, when one examines aerial photographs of the middle Columbia one finds that this arrangement is not unusual in the Plateau, although we have no dating or careful reporting of the fact. Workers in the Southwest or Southeast, will, I fear, be somewhat perturbed by the meagerness of the data on which historical schema in the Northwest are founded.

Swanson has called to my attention an error in make-up in the tabular presentation of the Columbia-Fraser chronology presented by Tuohy on page 44. In the column in the bottom half of the table headed "McNary," the Walula Gap Phase with Rabbit Island I and Rabbit Island II should be raised to coincide with Cayuse Phases I to III. Also, Moses Lake extended from somewhat earlier than Cayuse I to somewhat later.

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Arroyo Sequit: Archaeological Investigations of a Late Coastal Site in Los Angeles County, California. Freddie Curtis. Archaeological Survey Association of Southern California, Paper No. 4, Los Angeles, 1959. xiii + 169 pp., 124 figs., 9 pls., 6 tables, map, foldout chart. $3.50.

Considering the great amount of unpublished or partially published data from excavations of archaeological sites on the South Coast and Channel Islands of California, the present report must be looked upon as a model of careful and complete recording for the region.

The larger part of the paper is devoted to description and distribution within the Arroyo Sequit site of artifacts recovered during two salvage excavations, by the University of California (Los Angeles) and the Archaeological Survey Association of Southern California. In addition, specimens recovered at the site during the past 20 years by several private collectors have been briefly reviewed. Prominent among the latter are elaborately carved and shell-inlay decorated effigy specimens, pipes, tubes, and cups of steatite and serpentine, reported for the most part to have been collected by A. R. Sanger. Other sections (and five appendixes) dealing, for example, with the ecological setting of the site and including a comparative trait list for late Southern Californian archaeological sites and areas serve to present as rounded a picture as possible of the life of the late prehistoric occupants of the site.

Good reasons are presented for assigning the site to a late prehistoric culture period, with native occupation continuing into the historic period and even into the Mission period in California. It is not known, however, why the author has chosen to list (p. 129) "evidence of basketry" as a trait diagnostic of the protohistoric period and historic period. There is evidence both from the South Coast – Channel Island region and from Central California that wicker and twined baskets, respectively, were manufactured in earlier periods.

The investigation of the Arroyo Sequit site has produced one result that is worth special note here. This is that in all of the controlled salvage excavation no material similar to the extraordinary steatite and serpentine artifacts collected by A. R. Sanger for various agencies, including the Heye Foundation, was found, even though the more recent excavations took place in sections of the burial ground supposedly not touched by Little and Sanger from 1939 to 1944.

The Arroyo Sequit monograph is the most ambitious work yet published by the Archaeological Survey Association of Southern California. The photographic reproduction in the paper is good, and the profuse line drawings by the author are rough but entirely adequate to the purpose. In all, it represents a solid contribution to the archaeology of the region, and demonstrates, even at such a late date, that valuable, substantive material is still to be obtained in this archaeologically rich, but much ravaged, region.

A. B. Elsasser
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Berkeley, Calif.

Foote Canyon Pueblo, Eastern Arizona. John B. Rinaldo. Fieldiana: Anthropology, Vol. 49, No. 2, Chicago Natural History Museum, Chicago, 1959. 149 pp., 62 figs., 3 tables. $4.00.

This is the final report of a series covering 12 seasons of work in the Mogollon culture area, in the vicinity of Reserve and Pine Lawn in western New Mexico and adjacent Arizona. Paul S. Martin, director of the project, presents the Foreword, in which he restates their original intentions: (1) "the search for and recognition of consistent interrelationships between cultural phenomena in order to establish similarities that might recur within or across cultural boundaries, or indeed even in historically separate areas"; and (2) "an interest in the historical approach." Rinaldo prepared the materials and arranged the present publication in a highly commendable manner. The text is concisely written, unencumbered with statis-

tics — which are set forth in readily usable tables. Excellent photographs depict the setting of the Foote Canyon site, some 20 miles due west of Reserve. Other illustrations show excavation details and architectural features, pottery, and all classes of artifacts with equal clarity.

The Introduction includes exact location of this late pueblo ruin, and brief statements concerning the geology, climate, water, biota, and background of the area and of previous explorations. Then follows the Description of Architectural Details, with a word as to excavation procedure. The site consists of several house masses or blocks of rooms, clustered in the vicinity of a large rectangular kiva which is at the foot of the mesa on which the pueblo was located. There are approximately 50 rooms in all, built around a quadrangle. The stone masonry is of two types: (1) mud rubble of unshaped field stones or larger boulders of assorted sizes; and (2) large boulders alternating with courses of small, thin slabs. The ruin excavated was the largest of a group of five. An important fact brought to light is that the quadrangle was roofed, thus forming a covered plaza, which Rinaldo believes was possibly used as a community ceremonial room. Such covered plazas have been suspected but unproven, hitherto. Entrance to this plaza was through a portal located in one corner of the quadrangle and by doors in the outer tier of rooms. Subheadings give ready reference to walls, plaster, openings, floors, bins, pits, roofs, and other features. Two types of rooms were determined: (1) large rooms with firepits, ventilators, and milling centers; (2) small rooms, without floor features, which usually had a few manos on the floor.

The third section deals with Pottery. Some 20 whole or restorable vessels and 19,078 potsherds were recovered during the course of the excavation of 12 rooms and the plaza. These were sorted and classified into 40 established types and their variants, and are discussed with regard to use, construction techniques, sequence of types, the painted types, and the significance of the painted pottery types. Artifacts are presented in the fourth section under subheads such as milling stones, grooved tools, projectile points and blades, bone implements. In conclusion to this section, Rinaldo states: "The artifacts as a whole reveal an economy which is rooted firmly in the Mogollon tradition but which has been modified by late internal developments and by outside influences in the direction of the historic Western Pueblo culture." During the later phases, more kinds of ornaments were found, including copper bells. The Estimated Chronological Position of Foote Canyon Pueblo is set forth in the fifth section, considered under the headings of stratigraphy and architecture, with artifacts, seriation, and cross-dating. These indicate that the excavated rooms may date from A.D. 1245 to 1325, and the plaza, as late as A.D. 1350.

The sixth and final section is General Summary, Conclusions and Conjectures. With the pottery, slight changes were indicated by the shift to polychrome and black-on-red wares, while the basic Mogollon brown utility wares persisted. Stone and bone tools reflected a similar situation; there was a minor amount of change and a considerable degree of conservatism. The excavations made evident that the Foote Canyon Pueblo had been abandoned gradually and in an orderly fashion. Rinaldo says: "The former occupants took almost everything with them that could be carried easily or that was of any value. The objects left in the rooms were arranged carefully and there were no burned rooms or evidence of warfare. It was clear that the occupants had not been driven out of the village by an attack." However, a burial found with an arrowhead in the shoulder blade indicates that strife was not unknown. Frequent addition of rooms, alterations in room sizes, changes in floor levels and related features, are taken by Rinaldo to indicate a general dissatisfaction with the status quo and a desire for change. The people divided and, then, during the middle of the 14th century, abandoned the pueblo entirely. A tentative trait list of the Foote Creek phase is given in Appendix A, adding further to the usefulness of this publication. This is a timely volume, especially valuable in tracing the movements of peoples throughout the Southwest during the 14th and 15th centuries.

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An Inventory of Prehistoric Sites on the Lower San Juan River, Utah. William Y. Adams and Nettie K. Adams. Museum of Northern Arizona, Bulletin 31 (Glen Canyon Series No. 1), Flagstaff, 1959. vi+53 pp., 15 figs., 13 tables, 8 maps. $2.75.

For many years the San Juan River Valley has been considered one of the best known areas, archaeologically speaking, in the entire Southwest. However, in spite of the prodigious bibliography pertaining to that area, new publications continue to show that there is still much to learn about the San Juan Anasazi. The present publication and others which will follow as part of the survey and excavation program of the Upper Colorado Storage Basin will certainly be counted as significant contributions to the archaeology of the area.

Although faced with reporting a survey of a relatively unproductive section of the San Juan River, the authors of this handsome report have made the most of their meager data. The survey of 71 miles of the lower San Juan yielded only 83 small sites although the area is surrounded by the developed cultures of the Mesa Verde and Kayenta areas with their large spectacular ruins. The lower San Juan is truly a marginal area almost in the center of the Southwest, and is typified by crude, boulder masonry rooms (rarely more than three to a site), rockshelters, caves, and open lithic sites concentrated in a relatively few areas where accessibility from the uplands and arable canyon bottomland are the factors determining settlement pattern. The border-line nature of the area is further indicated by the predominance of Mesa Verde sites on the north side of the San Juan and Kayenta sites on the south side of the river. There were no indications that these sites served defensive purposes.

On the basis of ceramics the authors suggest that the lower San Juan sites were all occupied during the brief period from A.D. 1150 to 1200 — an unbelievably short interval. The presence of the relatively late pottery types Tsegi Red-on-orange, Tsegi Black-on-orange, and Dogozshi Polychrome on sites yielding earlier types in the Tsegi Orange and Tusayan white wares leads the authors to assume that all of these types were manufactured and used simultaneously in spite of the chronological distinctiveness shown in larger sites away from the lower San Juan. It seems unlikely that these later types developed in the lower San Juan area as the reasoning of the Adamses would imply. It is hard to believe that these seasonal inhabitants would devote much time or thought to developing new pottery designs and decorative combinations while eking out an existence in substandard habitations and with an unsophisticated array of stone, bone, and wood artifacts. Instead, it is possible that these late pottery types should not be thought of as part of a single assemblage representing one brief occupation, but rather an accumulation of potsherds resulting from seasonal use and re-use by many individuals over an extended period of time — perhaps from as early as A.D. 1050 to as late as A.D. 1300. Thus, the refuse of an 11th century squatter would be mixed in with the thin refuse of a 13th century seasonal farmer.

Another problem noted in this report deals with the perplexing situation where an area was occupied, then abandoned for a long period only to be re-occupied again. In recent years much space in Southwestern archaeological literature has been devoted to analysis of settlement patterns, while lack of settlement is generally ignored. Perhaps this latter approach, as briefly treated by the Adamses, will receive greater emphasis in the future.

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Two Archaeological Studies in Northern Arizona, The Pueblo Ecology Study: Hail and Farewell and A Brief Survey Through the Grand Canyon of the Colorado River. WALTER W. TAYLOR. Museum of Northern Arizona, Bulletin 30, Flagstaff, 1958. 30 pp., 1 map, 7 tables. $1.00.

This small bulletin records the "life and death" of one archaeological project and a preview of what might be done in another. In his *Hail and Farewell to the Pueblo Ecology Study*, Taylor has the courage to set down the details which led up to an unsuccessful attempt to answer the question of whether the "alleged Great Drought of 1276 to 1299 A.D. affected the Anasazi culture of northeastern Arizona, and if so, in what way."

The Ecology study might be termed a stream-of-consciousness monograph, for Taylor takes the reader step by step through the background and formulation of his problem and his futile search in three field sessions for an archaeological deposit dry enough and extensive enough to answer the specific questions he wanted to ask of it. His major conclusion is that so much vandalism has been perpetrated on the cliff-house sites in this area that it is already impossible to solve satisfactorily a problem of this kind. This certainly is a sad commentary on the state of archaeological material in a section of the Southwest which still has many major unsolved problems surrounding its cultural development. Perhaps, however, Taylor was looking for too ideal a site and was not willing to take his answers when and where he could. He thinks not. Finally, he turns to the problem of open sites and cliff dwellings and their relation to the problem of nomadic raiders. On the basis of an analysis which appears to be the by-product of his original project, he feels that there is little evidence to indicate that raiding was the cause of cliff-house occupation. Without passing judgement on the validity of his conclusion, I feel that his analysis is most superficial.

Taylor's second paper is a report of an archaeological survey which was the result of a six-day, 300-mile, Chriscraft trip down the Grand Canyon. Unfortunately, the speed of the trip left little time for field work. Seven sites and what may have been the famous Hopi salt mine were located. With one exception they are all in the eastern half of the canyon, which in part reflects the nature of the canyon and in part the time available for surveying. A short description of each site is accompanied by a table of sherd numbers, but there are no structure drawings nor is there a map. A great deal of time could have been saved and more sites visited if prior to the trip, the location of sites in the canyon had been obtained from the records of the Grand Canyon National Park. Based on his meager sample of sherds, Taylor hypothesizes a small Pueblo I occupation of the canyon area immediately adjacent to the river, then a growing population in Pueblo II times, with an eventual abandonment of the region in Pueblo III times. His report would have been greatly enhanced, and possibly the hypothesis strengthened, if he had included sherd collections from river sites collected on previous river trips. Such sites are recorded at the Museum of Northern Arizona and Grand Canyon National Park. In summation, this archaeological report is an interesting but brief glimpse into a still largely untouched area of Southwestern prehistory.

DOUGLAS W. SCHWARTZ
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Guide to the Identification of Certain American Indian Projectile Points. ROBERT E. BELL. Oklahoma Anthropological Society, Special Bulletin No. 1, Norman, 1958. 104 pp., 48 pls. $4.00.

This convenient "Guide" illustrates, describes, and discusses the distribution, age, and cultural affiliation of 50 projectile-point types which occur (in some parts, at least) of Texas, Oklahoma, the Plains, and Midwest. The form of presentation is much like that which Suhm and I

used in *An Introductory Handbook of Texas Archeology* (1954) except that the format is larger (8½ x 11 inches), ink sketches are used instead of photographs, and the sample specimens are reproduced actual size. The latter point is a distinct advantage to those just learning to identify projectile points, for in the Texas "Handbook" we found that the half-size photographs were often misleading to the uninitiated.

Of the 50 types presented by Bell, the data on 35 are copied or paraphrased from the Texas "Handbook." The remaining 15 are taken from several other sources. As the "Handbook" has been out of print since 1957, the present "Guide" partially makes up for this need. Some of the types chosen do not occur in Oklahoma, others only rarely or uncertainly. Thus the choice is not directed primarily toward students of Oklahoma archaeology, but rather is heavily dependent upon the research already done in the Texas "Handbook" and in some localities of the Plains and Midwest. Bell states: "The types which are included herein . . . are merely those on which work has been completed up to this date," but this is true only in the sense of work completed for the "Guide," not on projectile-point research in the southern Plains and surrounding areas. The 50 types were selected, then, not because of any central area or problem, but "because the illustrations were completed."

The illustrations were selected to show "typical" specimens rather than ranges in variation. While useful for the beginner, such selection avoids the real problem of typological analysis, namely, the unraveling of multitudinous overlappings in size, proportions, and workmanship which one faces on every hand in the laboratory.

A classic example of *research progress* — the kind which we hoped to encourage with the Texas "Handbook" — can be seen in the projectile-point analysis by Webb in "Poverty Point, A Late Archaic Site in Louisiana" (James A. Ford and Clarence H. Webb, 1956, *Anth. Papers of Amer. Mus. of Nat. Hist.*, Vol. 46, Pt. 1, pp. 50–76). Here we see a painstaking attempt to see how many of the Texas types can be identified in a neighboring state, with the result that while some of them can be, others show a somewhat different range of variation, and still other material must be arranged into new types of tentative inclusiveness. Webb handles this problem of comparison, testing, and new definition in masterful fashion.

Bell's "Guide," on the other hand, by accepting the definitions and ranges of variations already worked out in other regions, tends to make typological analysis seem too easy or too mechanical rather than an ever-continuing research problem, needing only a few clues to "identification." For example, while slight differences are apparent in the illustrations selected for the Harrell, Reed, and Washita "types" of triangular arrow points, in practice one would have great difficulty in distinguishing them. Are they really three types, or two, or one — with some slight variations here and there? All in all, this compilation has utility for certain purposes, but it would be much more valuable if it included new and critical research in Oklahoma in comparison with surrounding states.

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Birdstones of the North American Indian. Earl C. Townsend, Jr. Indianapolis, 1959. x+719 pp., frontis. in color, 308 pls. (11 in color). $25.00.

This beautiful and lavishly illustrated book reflects the interests, appreciation, and scholarship of the author as well as the skilled editorial hand of Gayle Thornbrough, who has been responsible for the technical excellence of the publications of the Indiana Historical Society. Townsend's book contains just about everything one would want to know about birdstones. The first five chapters of Part 1 provide a definition of birdstones, a history of theories and speculations about their use, and a convincing case for the hypothesis that birdstones were parts of atlatl-handle grips. In Chapter 6 the author painstakingly describes the cultural affinities of birdstones and correctly indicates that they belong to the Late Archaic and Early Woodland periods prior to the beginning of the Christian era.

The next five chapters deal with the manufacture of birdstones, their pattern of distribution, the aboriginal repair and salvage of broken birdstones, the people who collect birdstones, and fraudulent specimens. The chapter on frauds was particularly fascinating. After reading it carefully and looking at the illustrations, I easily was able to recognize the frauds in the collections of Chicago Natural History Musuem, two of which are illustrated in this section. It rather looks as if the major museums of the country, as well as many collectors, are loaded with fraudulent specimens.

Part 2 of Townsend's book is a pictorial catalogue of birdstones arranged by state of origin and category, representing most, if not all, of the major collections in the United States. Here are illustrated birdstones from eastern North America, particularly Ontario, Illinois, Indiana, Michigan, and Ohio; but all states and provinces that have yielded birdstones are represented.

This elaborate book was obviously produced as a labor of love, an act in keeping with the author's concept of the birdstone as an art object, and herein lies its appeal to the connoisseur. It should also appeal to the professional archaeologist, for in the pages of this book he can inspect more birdstones than he otherwise would see in a lifetime.

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Southeastern Indians, Life Portraits: A Catalogue of Pictures, 1564–1860. Edited by EMMA LILA FUNDABURK. Luverne (Alabama), 1958. 136 pp., 357 illus. $7.50.

This is a companion volume to the author's earlier, *Sun Circles and Human Hands*, a picture book of Southeastern archaeology. Essentially, this present volume is a picture book of Southeastern Indians as depicted by early explorers and artists from the 16th century to 1860.

The book consists of two parts, the first of pictures arranged chronologically and by artist, when known. With each reproduction is given the title, artist, date and present location of the original or source of the reproduction. In the second part, "Notes on the Illustrations" are given longer original explanatory captions, and various other data such as interpretations of the pictures, discussion of their date, place of painting, artist, and previous reproductions. This material consists almost entirely of quotations from literature and from correspondents with virtually no new discussion; however, much interesting information is included.

Many of the pictures, such as those by Le Moyne and White, have been published often; others, such as those of De Batz, are known to regional specialists, but Fundaburk has managed to dig out from unusual places some interesting depictions which are virtually unknown. However, the real value of the book is having all of this material in one place. With few exceptions the illustrations are well reproduced and on a scale adequate enough for the examination of ethnographic detail.

The earlier depictions are perhaps of most interest to the archaeologist. These tend to show ethnographic detail of value in archaeological interpretation, while most of the later pictures are portraits. This book can be considered a useful addition to the literature and worth at least an examination by all readers of this journal.

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River Basin Surveys Papers, Numbers 9–14. Edited by FRANK H. H. ROBERTS, JR. Bureau of American Ethnology, Bulletin 169, Washington, 1958. ix+392 pp., 13 figs., 73 pls., 9 maps. $3.25.

Paper Number 9, *Archeological Investig[illegible] Heart Butte Reservoir Area, North Dako[illegible]*, F Cooper, is the final report for the Heart Bu[illegible] Rese[illegible] some 50 air miles west-southwest of Bismarck, Dakota. Seventeen sites were located within the r[illegible] area. The surface surveys of 16 are reported briefly appendix. The excavation of 32GT1, the Koehle[illegible] occupies the greater part of the report. The field work is clearly presented, illustrated by two maps, two figures, and supplemented by 25 photographs. No structures or storage pits were identified; evidence of horticulture was lacking. Fragmentary bison bones were abundant, suggesting the extraction of bone grease. No European materials were found. Comparison of the artifacts, especially the pottery, with materials from Double Ditch and Slant Village suggests to Cooper that the occupants of the Koehler site were Mandan or some closely related group, tentatively dated early in the 18th century. Cooper's final and important point is that "there seems no doubt that the western tributaries of the Missouri River constitute a fruitful field for archeological research."

Paper Number 10, *Archeological Investigations at The Tuttle Creek Dam, Kansas*, by Robert B. Cumming, Jr., is devoted primarily to a report of the excavation of Site 14PO14, the Sweat Bee Mound, on the axis of the Tuttle Creek Dam, near Manhattan, Kansas. The relevant field data are set out in the text, supported by two maps and 11 well chosen photographs. Consideration of these data led Cumming to a tentative identification of the site as "an attenuated form of the Woodland burial mound building complex of Missouri." An appendix presents a thorough analysis of the six adult burials from Sweat Bee Mound. Cumming concludes that "the 14PO14 crania fit in morphologically with the early Woodland Otamids." This report is an important addition to the scanty published data on Plains skeletal remains. A supplemental note by William M. Bass adds observations and corrects certain age estimates. Bass comments "as a result of this study, I feel that Cumming's report is well done," and I agree.

Cumming also reports sampling two sites on the dam axis. Site 14PO12, the Spillway site, may be related to the Glen Elder focus of the White Rock aspect. Site 14PO13, the Reany site, included shell-tempered pottery and four small pieces of metal. Cumming states, "It seems quite probable that the Reany site is a protohistoric or early historic Kansa site." Preliminary trenching at 14RY10, just south of the dam, disclosed the floor of a small earth lodge. Investigations, halted by the end of the fiscal year, were completed by two volunteer parties, which I directed. Cumming suggests an assignment to the Table Rock focus of the Nebraska (now Aksarben) aspect, and I concur.

In Paper Number 11, *The Spain Site (39LM301), a Winter Village in Fort Randall Reservoir, South Dakota*, Carlyle S. Smith and Roger T. Grange, Jr., submit a full-scale report of the field and laboratory work of the University of Kansas Summer Field School in 1953. Three sites were investigated on the west side of the Missouri near the mouth of White River. The Spain site, 39LM-301, provided the major portion of the data. Investigations of 39LM47, the Clarkstown site, and of 39LM39, the Deerfly site, demonstrated two components at each site. The earlier is closely related to the Spain manifestation and the later can be attributed to the Dakota, late in the 19th century. At the Spain site, two round earth lodges, and a probable third, were identified, as well as a small midden and other features observed in exploratory trenching. The relevant data are well organized and presented in full. After comparison with the large amount of material now available for the Middle Missouri, the authors attribute the occupation of the Spain site to the Shannon focus of the Chouteau aspect, with a tentative

date of A.D. 1600±50 years. Subject to further research, the authors indicate that the Chouteau aspect may represent the culture of the Arikara and Pawnee at that time.

These are well written and important papers. Cooper submits a final report for Heart Butte Reservoir archaeology; Cumming adds to our knowledge of Plains skeletal material; Smith and Grange contribute a provocative discussion of a complex Middle Missouri problem. These papers deserve more attention than the space allotted will permit.

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River Basin Surveys Papers, Numbers 9–14. Edited by FRANK H. H. ROBERTS, JR. Bureau of American Ethnology, Bulletin 169, Washington, 1958. ix+392 pp., 13 figs., 73 pls., 9 maps. $3.25.

Three of the papers in this volume deal with archaeological sites in the southeastern United States:

The Wilbanks Site (9CK–5), Georgia, by WILLIAM H. SEARS (Paper No. 12); *Historic Sites in and around the Jim Woodruff Reservoir Area, Florida-Georgia,* by MARK F. BOYD (Paper No. 13); *Six Sites near the Chattahoochee River in the Jim Woodruff Reservoir Area, Florida,* by RIPLEY P. BULLEN (Paper No. 14).

The Smithsonian Institution and Frank H. H. Roberts, Director of the River Basin Surveys, are to be complimented on the quality of these reports, which are the result of the cooperative efforts of the Smithsonian Institution, the National Park Service, the University of Georgia, the Florida Historical Society, and the Florida State Museum.

The work of the University of Georgia at the Wilbanks site (9CK–5) was the first major salvage excavation in the Allatoona Reservoir on the Etowah River in northern Georgia. Reports on the later Smithsonian excavations at Allatoona by Carl F. Miller and me await publication. The Wilbanks site is in a broad bottomland of the Etowah valley 18 miles above the famous Etowah Mounds near Cartersville. The locality was marked by a low earth mound, which Wauchope had briefly investigated in 1938, and by scattered surface materials brought to light by cultivation and in river washouts. Sears's work was concerned with the mound, found to comprise the remains of a fallen earth-covered building. Large logs had been laid to form a square 45 feet on each side. Rafters across these had their inner ends touching at the roof peak. A buttress of gray clay was cemented around the large logs and the bases of the rafters. The roof had a covering of earth. After this building had fallen, occupation debris accumulated on it. The strata consisting of the premound levels, the floor of the building, and the later midden yielded substantial samples of Southern Appalachian paddle-stamped and other pottery which have given us one of the surest stratigraphic sequences in the Etowah valley. As usual at later southeastern sites, artifacts other than pottery were infrequent.

Phase A, the earliest division of a continuous occupation, is based on the study of more than 4000 sherds from the premound level. This defines the Etowah II period of a scheme which Sears and I devised. The subsequent division, Phase B, defining Etowah III, represents some 8000 sherds from the upper premound zone. From the floor of the building, belonging to the latter part of this phase, came more than 6000 sherds. The subsequent Etowah IV period was not represented. There was a reoccupation during the Wilbanks period, defined here by Sears, then another lapse of occupation, and finally a light occupation during the Lamar period. The pottery of Phases A and B (Etowah II and III) shows continuity and change. The stamped motif called the two bar diamond, the best known variety of Etowah Complicated Stamped, is most numerous in both phases. The earlier phase, however, is distinguished by the ladder based diamond motif (20% of decorated sherds). In the later phase is the filfot cross (14% of decorated sherds). Another design, the line block, is infrequent in the earlier phase (3%) but strong in the later (21%). Similarly, shell tempering occurs in 28% of undecorated sherds in Phase A but only 6% in Phase B. The minority type Hiwassee Complicated Stamped (like Etowah Complicated Stamped, but shell-tempered) is only present in Phase A. Hiwassee Red on Buff is only present in Phase B.

Pottery, minor artifacts, and the structure in the mound show a mixture of Mississippian and indigenous culture elements. Specific connections with eastern Tennessee are expressed by the occurrence of Hiwassee Complicated Stamped and Hiwassee Red on Buff. Other ceramic and nonceramic items, such as notched added rim strips, appliqué human face effigies, strap handles, pipe and projectile point types, and pottery discs, are found not only in eastern Tennessee but at many Mississippian sites. Sears's interesting discussion suggests that the importance of earth lodges in the Southeast has been neglected in the past.

This is a clear report. Only when the discussion turns to questions of relationship to other sites does confusion creep in. I am at a loss to understand Sears's view that the Savannah period which I proposed for the Etowah valley was erroneously based on Wilbanks materials. It was only after I advised him that his own pottery was not Savannah Complicated Stamped that he devised the name Wilbanks for it. Indeed, as Sears admits (pp. 174, 176), Savannah Complicated Stamped *does* occur in the Etowah valley. In fact it is the majority type at pure sites and levels (9 BR 60–A, 9 BR 62–A, 9 CK 85–A, B, C, 9 CW 106–C, 9 CK 78, 9 CK 52–B, 9 CO 85, 9 CO 57, and 9 CO 51).

Sears offers another objection to the Savannah period based on the grounds that only the type Savannah Complicated Stamped occurs in the Etowah valley but not the companion types found on the Georgia Coast. Cavils of this kind arise when there is confusion between the terms "period" and "culture." Surely Sears does not expect all

the sites of his Mature Mississsippi period to have identical assemblages; or does he? A Savannah period in northern Georgia implies nothing more than the kind of contemporaneity indicated by the use of Savannah Complicated Stamped in both areas. It is handy to be able to remember that the things happening on the Georgia Coast during the Savannah period were roughly contemporary with the entirely different things going on in northern Georgia during the Savannah period there. We can hardly expect these two areas to be culturally alike.

In the introductory and concluding sections of his report Sears stresses the importance of his concept of a "Mature Mississippi . . . culture type and period . . ." which is based on the Kolomoki–Weeden Island manifestation and the Etowah sequence. Briefly, the Mature Mississippian culture type and period includes a "peak of artistic and ceremonial development" represented by the Southern Cult at the Etowah site and which Sears repeatedly assigns to the "Etowah Period." According to Kelly and Larson (*Archaeology*, Vol. 10, No. 1, 1957) the peak of ceramic development (Etowah II and III) clearly precedes the peak of Southern Cult development (Etowah IV and most strongly in the succeeding Wilbanks period). There is then some question of the "maturity" of the Etowah materials in the terms of Sears's own definition. More than this, it is difficult to see how Mature Mississippian can constitute a period if this is to include Kolomoki–Weeden Island in southwest Georgia. This precedes the Southern Cult at the Etowah site by about 500 years.

It is understood that when one constructs a culture type to elucidate Mississippian history he might be allowed to take certain liberties depending on the problem at hand. Unfortunately the Kolomoki site has no relevance to the developed Mississippian cultures, and in fact Kolomoki is not Mississippian at all. Sears is on surer ground with the Etowah sequence. Although of local derivation it apparently fell under strong Mississippian influences. One hopes, however, that the Etowah I, II, III, and IV periods do not become taxonomically pigeonholed as Mississippian. There are other ways of looking at this mixed Mississippian–Southern Appalachian continuum which profitably emphasize the other side of the coin — its local roots and the context into which these Mississippian cultural elements were being lodged, a context I have elsewhere referred to the Southern Appalachian Tradition.

The papers by Bullen and Boyd are two of a projected four on the archaeology conducted by the National Park Service, Smithsonian Institution, Florida State Museum, and the Florida Historical Society in the Jim Woodruff Reservoir area at the forks of the Flint and Chattahoochee rivers in southwest Georgia and northwest Florida. We now await only the Georgia reports which Carl F. Miller and I prepared.

The highlights of Bullen's work in the Florida part of the area include defining a late Archaic Orange period in northwest Florida with a radiocarbon end-date of 1200 B.C. ± 250 years. At the other end of the scale Bullen has defined a Lower Creek period marked by the pottery type Chattahoochee Brushed occurring with early 19th century English stoneware and blue-edged Staffordshire. The Orange period is interesting in showing widespread connections with other parts of the Southeast. "Circa 1000 B.C.," writes Bullen, "was not a period of isolation in the southeastern part of the United States." In addition to the fiber-tempered pottery, the presence of St. Johns Incised shows connections with the early chalky pottery of eastern Florida. The presence of steatite vessels, evidently from central Georgia or northern Alabama, is reminiscent of the widespread secular trade we know from Poverty Point in the lower Mississippi valley at about the same time.

The most important single site of the six excavated by Bullen (at least 60 were located in the original survey) was J–5 on a natural levee northwest of the Jim Woodruff Dam. A stratigraphic cut yielded sparse preceramic materials at a depth of 14 feet. Separated from this by a sterile zone was the late Archaic deposit already mentioned which included more stone tools than had been previously known from all other fiber-tempered pottery sites in Florida. In the upper levels of the excavation were Deptford and Ft. Walton occupations. As usual, the Deptford deposit offered little besides sherds. The Ft. Walton level contained considerable pottery and suggested the possibility of dividing this period into several phases. Four pits oriented to the cardinal points and full of charred corn invited comparisons with Creek ceremonial.

Another site, Ja–63, was a Kolomoki period habitation area with a good sample of pottery, a few other objects, and some faunal remains. Bullen writes of his perplexity concerning the disagreement between Sears and me as to the chronological position of the Kolomoki period, but tends to agree with Sears that it falls within the Mississippian time span. He feels that "the zoned location of the stamp on the necks of some Kolomoki Complicated Stamped containers, the Mississippian shapes of many of the vessels at the Kolomoki site, the tendency of the 'free' incised decoration to be in a band below the lip and sometimes to be suggestive of Ft. Walton designs are all relatively late traits." In order to put Kolomoki subsequent to Weeden Island and equivalent to Mississippian times, Sears was forced to discount Willey's stratigraphic evidence at Mound Field in Florida, my own stratigraphic sequence at Fairchild's Landing in Georgia, and a stratigraphic sequence at the Kolomoki site itself.

Bullen notes that the radiocarbon date from Ja–63 supports the earlier placement for Kolomoki, and so for that matter have the dates obtained subsequently. This is the Kolomoki period, discussed earlier, which Sears used to define his Mature Mississippi culture. Bullen suggests that if Kolomoki is indeed older, the alternative will be to place the origin of Mississippian-shaped vessels at the Kolomoki site. This is not the alternative, but Bullen has put his finger on the essential point, namely that the supposedly Mississippian shapes of Kolomoki vessels, as well as the platform mound, may reflect ele-

ments, probably Gulf, which elsewhere were involved in the early development of the Mississippian Tradition. This promising line of investigation would not be possible if we were to assume that the possible Mississippian elements at such sites as Kolomoki must necessarily be late.

A further point of interest in Bullen's report concerns the presence of sites that had Wakulla Check-stamped as the major or only decorated type. I have found and excavated others which define the Wakulla Period, that should precede Ft. Walton in the area.

The various participants in the Jim Woodruff investigations were fortunate to be associated with Mark F. Boyd, M.D., who here reports on historic sites in and around the area. Boyd, long known for his extensive studies in the history of Florida, took for his range of time everything up through the American Civil War. This was a quite eventful history and it would repay archaeologists to read his report. Boyd personally inspected every site he writes about and corralled all documentary sources. He questioned all of us about our own findings and where possible combined these with his own data. The complexity of the events of the Lower Creek Period, for example, affords a striking contrast to the apparent simplicity of the archaeological situation where all that is left at many small sites are a few sherds of Chattahoochee Brushed pottery, Staffordshire, an occasional bit of green glass, or a gunflint. One shudders to think of the situation the archaeologist would be in if the ravages of time had not made his work so much easier. The historian's history is more complex.

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Preliminary Archaeological Investigations in the Sierra de Tamaulipas, Mexico. Richard S. MacNeish. Transactions of the American Philosophical Society, n.s., Vol. 48, Part 6, Philadelphia, 1958. 210 pp., 51 figs., 31 tables. $5.00.

This is a prodigious and wondrous work. MacNeish has performed an enormous labor both in the field and in the laboratory and has opened to archaeology a new territory of considerable importance and impressive resources. In many regards, his cultural vision is excitingly broad and, as usual, his energy, enthusiasm, and good humor stand out for all to see. It is all the more disconcerting, therefore, to find that certain of his basic theoretical premises and the resultant uses of empirical data tend to make some of his more central conclusions highly suspect, if not downright impossible to believe. And an astonishing deficiency, in his presentation, of what is usually considered to be minimal scientific precision does not give the reader much added confidence to counterbalance these suspicions.

MacNeish opens with a section on Problems and Method, and it is pleasing to report that throughout the entire publication he has provided reasoned explanations of his aims and procedures, so that the reader is made aware of these basic data which, in most archaeological reports, are shamefully neglected. If for no other reason (and there are actually many other reasons), this monograph is a valuable contribution to the literature. I look forward to assigning it to advanced students as an exercise in the analysis of archaeological theory and method.

Following this section, is one on the area, including descriptions of the environment, the ethnohistory, and the exploration and conquest of Tamaulipas. What is more to the point, MacNeish actually uses these data in his later interpretations. Detailed sections on the excavations, the surveyed sites, and the artifacts follow. The final section is titled Conclusions and brings together the significant aspects of the foregoing substantive information together with comparative studies of pertinent cultural complexes in Mexico and Texas. Chronology is also treated, both relatively and absolutely, using radiocarbon dates and seriated data from Tamaulipas and other regions. This section also includes a great deal of hitherto unpublished, empirical information (on northern and southwestern Tamaulipas) which, it seems to me, would have better been presented in a separate section and not in the Conclusions, which are usually expected to be synthetic and generalizing in nature.

From his investigations, MacNeish sets up a sequence of one lithic complex and eight cultural phases. These are arranged in chronological order extending from before 10,000 B.C. (geological date), through radiocarbon dates of 7320 and 2500 B.C. and A.D. 1292, to historic times (from Spanish goods found in the Los Angeles or eighth phase). Inferences based on comparative typology, particularly of the lithic industries of Texas and the ceramic industries of the Huasteca, are used to corroborate the sequence in Tamaulipas. He also adduces data from climatic and ecological changes and the local geological work of de Terra and Glen Evans in explanation of dating and cultural ecology.

The sites which MacNeish found include caves, open camp sites, villages with house platforms and evidence of wattle-and-daub structures, and what may be called towns with house platforms, formal plazas, pyramids, and in one instance a ball court. Artifactual material from the early lithic complex and his four earliest phases is largely of stone; agriculture appears first in the La Perra (third) phase and pottery in the Laguna (fifth) phase of his sequence. Special mention should be made of the finds of domestic plants, particularly of primitive maize, which MacNeish found in deposits attributed to the La Perra phase dated around 2500 B.C. These specimens have aroused much interest and have been the basis for important studies on early American agriculture by Mangelsdorf, MacNeish, and others.

Throughout his study and especially in the summary sections, he has used evidence from many sources and many aspects of his material to support inferences as to the life of the people and the historical and cultural significance of what he found. He has, in a word, employed a conjunctive approach and, whether we can agree with all his conclusions or not, the point is that he has tried

and, it may be added, succeeded in a great many cases. In his last section, which is entitled Problems and Speculations, his viewpoint is broadly anthropological in that he has made statements and asked questions which indicate his interest in the study of culture itself, as exemplified and particularized by his Tamaulipas material. To me at least, this is stimulating and cause for sincere congratulation.

Within the stipulated limits of this review, it is completely impossible to do critical justice to a work such as this. A reasoned and documented analysis, bringing out the points of strength and weakness, would involve many pages of text. MacNeish's report deserves such a treatment because of its theoretical and substantive implications and particularly because, contrary to usual practice and to his great credit, he has written his report so as to provide the means to conduct such an analysis. However, although a proper job is impossible here, I must discuss at least some of those disconcerting premises and uses of data which I mentioned in the first paragraph.

I have found it most difficult to accept the cultural units which MacNeish sets up in this report because the picture he presents is so neat and simple in derivation, yet so full of complex implications in final product. My own experience in very comparable sites has produced much more complicated stratigraphy, many more seriation problems, and far fewer cultural isolates. But because I feel that MacNeish's evidence deserves serious consideration, I have returned again and again to the analysis, trying to probe deeper into his procedures and premises and thus to uncover the causes of my feelings of doubt and malaise. I believe that I now have the answer, or at least part of it.

MacNeish seems to have made a conscious or unconscious effort to simplify his cultural synthesis and, as a result, has oversimplified. He now presents a picture which has a multitude of individual cultural units succeeding one another in a relatively simple progression through time, rather than a smaller number interacting among themselves and with their natural environment in a much more complicated manner. I believe this to be the result of certain of his basic, perhaps unconscious, premises which I shall try to elucidate. To him, a change in artifactual inventory (however slight) plus a difference in excavation unit (however determined) is reason to establish a separable cultural entity which he generally calls a "phase" and which he considers comparable with other cultural entities on a one-to-one basis and without further analysis or qualification. Further, I believe that MacNeish is too trusting, too depending, even naive, in his cross-cultural comparisons, as in his inferences based on the single, *so-called* Teotihuacan point found in Diablo Cave and in his identification of many Tamaulipas artifact types with those from other areas. The combination of these premises has permitted him to establish a sequence of cultural phases that he uses as a touchstone against which to compare and to place chronologically a host of other artifact congeries.

As it happens and as MacNeish himself admits in several places, this sequence of phases actually constitutes a continuum of artifact types which is broken significantly only by the advent of agriculture, pottery, and a very few other cultural traits. It is easily demonstrable from a purely methodological point of view that, if such a master sequence constitutes a continuum or a reasonable approximation thereto, any congeries of comparable traits, however determined, can be found to fit the continuum at some point or other within any required degree of exactitude which is not too demanding. This can be done for congeries associated in the ground and produced by excavation; it can be done for congeries picked from the surface; it can be done for congeries taken from completely random collections. The cultural and chronological significance of the placement is seen to depend, therefore, not so much on the fact of fit, but on the acceptability of the degree of fit and on the criteria by which the original sequence was established and by which the compared congeries were assembled.

It is in this regard that MacNeish has failed to provide satisfaction, with the consequence that his master sequence and his later comparisons are, at least to me, strongly suspect. To be specific, his master sequence was built largely on the basis of findings in Diablo Cave, but his determination of phases at this site is certainly questionable, if not definitely unbelievable. For example, it is evident that his Los Angeles phase consists of materials found either in the surface ash layer or, where this layer was not present, in a subjacent, arbitrarily delimited, 6-inch level of the succeeding refuse deposit. The artifacts found in the remainder of this refuse deposit are assigned to another phase entirely, evidently on the sole basis of mensurated, and hence arbitrary, stratigraphic position. Here we see a prime example of MacNeish's "one excavation unit — one cultural unit" premise.

We see the same at the other end of the stratigraphic column in the same site. The artifacts which were found in the silts lying on the cave floor at the back of the cave are said to represent the Lerma phase, while artifacts, which MacNeish himself says are identical with one exception to those designated as Lerma and which were found in the identical silts and also upon the cave floor but in the front of the cave, are set apart as representing what he calls the Diablo complex. The presence of a terrace gravel remnant within the silt deposit has evidently influenced MacNeish to separate the artifacts above and below into separate "cultural" units. This contention cannot be supported except on a rather rigid "one excavation unit — one cultural unit" basis. In other words, the unit of excavation, as determined by modern civil engineering, constitutes the basis for distinguishing "cultural" phases at this important site. This theoretical precept is brought to a *reductio ad absurdum* when, in his analysis of site Tm c 29, he attributes a different cultural complex or phase to each of his four, arbitrary, 6-inch levels of excavation and equates each without remainder or combination with previously-named complexes and phases from other sites.

It is further indicative of his "one congeries — one phase" premise that of the 62 sites reported from his surface survey only three are said to contain representa-

tions of more than one phase (three are given no phase designations at all). On strictly *a priori* grounds, I was skeptical that such an unusual situation existed. After an analysis of trait lists and percentages and when these had been compared with similar data from the excavated sites and from other congeries designated by the same phase names, my disbelief was overwhelming. MacNeish seems to make no allowance for small inventories, especially those gathered from surface sites, and his equations, if they are based on artifact types and percentages at all, are made with a latitude and inconsistency extending to the point of meaninglessness.

A final example of this "one congeries — one phase" premise is basic to his estimates of aboriginal community size and site occupation. Again on *a priori* grounds, I was skeptical that, at a distance of five to nine thousand years, there could be any validity in a statement that because a site was from four to one hundred feet in diameter and had refuse not over 3 inches deep the aboriginal community consisted of from two to ten people and the occupation lasted for from one day to six weeks. It is at once apparent that MacNeish has made no allowances for multiple, intermittent, sequential occupations over long periods of time nor considered the possibility that other factors than time might have influenced the depth and distribution of refuse, either originally or subsequently in the millenia after deposition. To him, if it is "one deposit" according to his own way of thinking, then it is "one phase," even "one band," even "one occupation."

In closing, I would like to offer some suggestions: the possibility that the cultural sequences and interactions in Tamaulipas are considerably more complicated than MacNeish has given them credit for being; the possibility, if not the probability, that the cultural deposits themselves, particularly those in the deeper cave sites, are not so "simple" as he says they are; the possibility that MacNeish's findings might more realistically and more significantly be grouped into three units (with subdivisions to be determined at some later time after more field and laboratory work?).

Suggested units	*MacNeish's phases*
I —	Los Angeles
II —	La Salta
	Esbalones
	Laguna
III —	Almagre
	La Perra
	Nogales
	Lerma

However, the assemblage designated as Lerma phase (together with the Diablo complex) may well constitute a validly separable unit, but at the moment this cannot be supported except by very few data and a "hunch."

This has been a challenging monograph to analyze. I have enjoyed it tremendously as an exercise in archaeological theory not only because of its content, which has been absorbing, but because of the wealth of ideas, premises both implicit and explicit, assumptions, theoretical postures, and examples of method that MacNeish has incorporated in this valuable work.

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Florentine Codex: General History of the Things of New Spain. Book 9 — The Merchants. Fray BERNARDINO DE SAHAGÚN. Translated by ARTHUR J. O. ANDERSON AND CHARLES E. DIBBLE. Monographs of the School of American Research, No. 14, Pt. 10, Santa Fe; University of Utah Press, Salt Lake City, 1959. 97 pp., 13 pls. $6.50.

In 1950 Anderson and Dibble initiated a monumental project to publish a paleography and English translation of the Nahuatl text of the Laurentian Library manuscript of the final 12-book version of Sahagún's encyclopedic survey of the native culture of the Valley of Mexico. This manuscript was apparently copied in Mexico in 1576–77 and taken to Spain by Fray Rodrigo de Sequera in 1580. With the appearance of this latest volume, only Books 6, 10, and 11, plus the introductory volume, remain to be issued.

Book 9, although not lengthy, provides materials of unusual ethnographic interest. It is divided into two distinct sections: (1) Chaps. 1–14: the merchants, *pochteca;* and (2) Chaps. 15–21: the goldworkers, *teocuitlahuaque, teocuitlapitzque;* the lapidaries, *tlatecque, chalchihuitlatecque;* and the feather workers, *amanteca.* This information was gathered and the original Nahuatl texts written in Tlatelolco probably in the period 1561–65; the valuable accompanying illustrations (those relating to the *pochteca* considerably closer to the native esthetic tradition) were probably drawn during the preparation of the 1576–77 copy.

Sahagún himself wrote a paraphrastic Spanish translation of all but Chaps. 16, 17 (pt. 2) 20, and 21. This version, derived from the *Manuscrito de Tolosa* (a MS of the Spanish text only of the whole work whose precise relation to that of Florence is still uncertain), has been published various times since 1830. The first direct translation (French) from the Nahuatl of the untranslated chapters, based on the earlier, somewhat more correct version in the *Manuscrito de Tlatelolco* (Academia de la Historia, Madrid), was published in 1892 by Seler (German version in 1927). Stemming from Seler's excellent effort have been Saville's 1920 English translation of Chap. 16 (republished by Lothrop in 1952) and Gott's 1938 Spanish translation of all four chapters. Recently (1952) Schultze Jena published the paleography and German translation of the Madrid version of the Nahuatl text of Chaps. 15, 17 (pt. 1), 18, and 19; and in 1956 Garibay and McAfee produced a direct Spanish translation of Chaps. 15–21 also based on the Madrid Nahuatl text.

The principal reason for the special attention paid this section of Book 9 lies in the descriptions that Chaps. 16,

17 (pt. 2), 20, and 21 provide of the actual techniques employed in their crafts by the gold, gem, and feather workers, respectively. The account in Chap. 16 of *cire perdue* casting is of particular importance as being the only detailed description of this complex metallurgical technique which has been preserved from any part of Nuclear America. The Dibble and Anderson translation, although quite close to those of their predecessors, has been considerably enhanced by their consultation with Maryon and Easby whose occasional valuable notes on ancient metallurgical techniques are included.

The fact that only one other direct translation of the Nahuatl version of the section of Book 9 which relates to the *pochteca* has ever been published (Schultze Jena, 1952, Nahuatl paleograph of the Madrid version and German) lends the Dibble and Anderson *Florentine Codex* translation particular value. Garibay has singled out this section for high praise on the basis of its superior literary quality.

With regard to the English translation as a whole, only one with a complete mastery of classical Nahuatl would presume to judge their effort; I will accordingly confine myself to a few general observations. Although I have never been entirely happy with the choice that was made "to parallel, in a sense, the late Renaissance feeling of Sahagún's Spanish through the employment of occasional archaisms, which might give it something of the atmosphere of the King James version of the Bible," it must be admitted that in this volume the use of flowery archaisms is kept to a strict minimum. However, occasionally the English is rendered in a somewhat awkward manner (for example, p. 7: "the noblemen, at this same time, went about with marvelous, precious capes tied on at the time when such great feast days took place as we came to during the year"), which probably could have been smoothed out without unduly sacrificing accuracy. One might also occasionally question their particular semantic choices, for example: "ocelot" for *ocelotl* (jaguar); "shell" for *coyolli* (although they recognize the preferable translation, bell, in a footnote); "ground drum" for *huehuetl* (upright cylindric drum). Although it is a useless criticism at this stage, many would doubtless have preferred to see both the paleography and the translation divided into numbered paragraphs, to facilitate reference (a positive advantage of the translations of Seler, Schultze Jena, and Garibay and León Portilla).

The footnotes, as usual, are minimal and largely involve variant textual renderings of the Spanish and the Madrid Nahuatl versions, plus specific botanical, zoological, and other identifications. The citation mechanics seem more laborious than necessary. A system using brief author, date, and page references, keyed to a list of works cited, would have substantially streamlined the footnotes. It is also surprising to note the puzzling continuance in this volume of their citation of Ternaux Compans's poor French translation of Tezozomoc's *Crónica Mexicana*, in the face of the availability of three editions of the work in its original language.

Lastly, a few remarks on the ethnographic value of Book 9. Although far from providing a complete picture of the "guild" structure and mercantile activities of the powerful, wealthy *pochteca* of the Valley of Mexico in the last few decades before the Conquest (Acosta Saignes's 1945 study is helpful here), Chaps. 1–14 do provide extremely useful information, of particular value to students of Mesoamerican economics. Worth noting especially are the historical reminiscences of the Tlatelolcan merchants, including their boastful claim, doubtless exaggerated, of having militarily subdued Anahuac Ayotlan (Chiapas coast) unaided; the vivid accounts of the great trade expeditions to the coastlands, including the involved preparatory and aftermath rituals and the detailed itemization of the goods traded on both sides; the elaborate masquerade techniques employed by the *nahualoztomeca*, "the disguised merchants," in gaining access to highland Chiapas (Tzinacantlan) before its conquest; the highly significant references to the private entrepreneuring of the Tenochca ruler, the judicial *fuero* of the merchant organization, and the existence of formal trading stations of 12 leading Valley of Mexico towns in the great commercial emporium of Tochtepec (Veracruz); and the detailed accounts of two elaborate feasts given by wealthy merchants on different occasions. The value of the information contained in Chaps. 15–21 has already been emphasized. Worth further mention, for its value to students of Tenochca economic structure, is the clear distinction made in Chap. 20 between the "state" and "private" *amanteca*.

As another milestone in one of the greatest ethnohistoric enterprises ever undertaken by American scholarship, the appearance of this latest volume in the series can only be enthusiastically greeted by all students of Mesoamerica's past.

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Das Memorial Breve Acerca de la Fundación de la Ciudad de Culhuacán und weitere ausgewählte Teile aus den "Diferentes Historias Originales" (MS. Mexicain No. 74, Paris). Domingo de San Anton Muñon Chimalpahin Quauhtlehuanitzin. Translated and edited by Walter Lehmann and Gerdt Kutscher. Quellenwerke zur alten Geschichte Amerikas, Band 7, Iberoamerikanische Bibliothek, Berlin; W. Kohlhammer Verlag, Stuttgart, 1958. xxxix+240 pp., 2 pls. DM 59 ($15.00).

In the present volume Kutscher has completed a task which Walter Lehmann began in 1907 and which, due to various circumstances, he left only partially completed at the time of his sudden death in 1939. The translation of the *Memorial Breve* and of certain parts of the *Diferentes Historias* from Nahuatl into German augments Mengin's splendid but untranslated facsimile reproduction of original manuscripts in the *Corpus Americanorum Medii Aevi*, (Vol. 3, Havniae, 1949).

Chimalpahin's works rank with those of two other eminent 16th century chroniclers, Ixtlilxochitl and Tezoz-

omoc, all of whom were Indian descendants of noble birth and who were familiar with the traditions of Mexico before the Spanish Conquest. Although he wrote about 100 years after the conquest, Chimalpahin was able to consult codices and manuscripts which were still in existence at that time, and he left in his *Memorial Breve* a chronological account for the years A.D. 670–1299 of the history of about a dozen localities. His work is now the only source which has been preserved of the early history of the region around the once important town of Chalco in the Valley of Mexico. Chimalpahin's collection of manuscripts, written partly in Nahuatl and partly in Spanish, came to the attention of several historians, among them Boturini, who mentioned them in his *Catálogo* (1746). In the 19th century they were taken to France by Aubin and eventually deposited in the National Library at Paris where the so-called Aubin-Goupil Collection now contains eight manuscripts attributed to Chimalpahin. Among them is the original draft of the *Diferentes Historias Originales*. This volume of 272 folios (MS Mexicain No. 74) is a compilation of more or less related chronicles written between 1612 and 1630. At some later date it was divided into eight "Relaciones." Of these, the first Relación, dealing with the Genesis, is of little interest for the anthropologist. In the second Relación Chimalpahin correlates the Mexican with the Christian calendar and gives a geographical description of the four continents then known. This Relación, breaking off in the middle of a sentence, is followed by the very important *Memorial Breve Acerca de la Fundación de Culhuacán*, which constitutes the remainder of this second Relación. The Memorial begins with the year A.D. 670 when the founding of Culhuacán supposedly took place. The author then presents a list of kings or chieftains which serves as a framework for the chronological account of events in the surrounding city states and emphasizes the occurrences at Amecameca, the author's birthplace. Of particular interest in the Memorial is the early history of the Azteca Mexica and the Olmeca Xicalanca. The Memorial ends in 1299 when the Mexica occupied Culhuacán (this occurred according to other sources in 1367) and concludes with a eulogy for Topiltzin Quetzalcoatl. Thus, in content, the Memorial has no close connection with the preceding text. It also differs in its presentation from the other pages of the Relación because it is neatly written and it appears to have been inserted in this place at a later time. The translations of the beginning of the second Relación (by Kutscher) and of the Memorial (by Lehmann) are published here for the first time. The third Relación relates the departure of the Mexica Azteca from Aztlán and gives a detailed history of their migrations and settlement in the Valley of Mexico. Selected passages translated by Lehmann appear in this volume. In the fourth Relación Chimalpahin deals with the genealogy of the ruling house of Amecameca and the destiny of the Totolimpaneca, a local group from that region. This has been translated by Kutscher; an abbreviated Spanish translation by Silvia Rendón was published in 1949 (*Anales del I.N.A.H.*, Vol. 3, Mexico). The fifth Relación is a valuable complement to the Memorial, treating of the early history of Chalco. It was translated into German by Mengin (1950); in this volume are selected passages translated by Lehmann. Of the incomplete sixth Relación, Lehmann translated only a short passage. In the seventh and eigth Relaciones the author describes various ethnic groups around Chalco and gives an account of the Spanish conquest of Amecameca, which was translated by Seler and incorporated in his Sahagún translation published in 1927. To summarize, this book contains translations of the second Relación with its Memorial, the complete fourth Relación, and selections from Relación III, V and VI. These are preceded by a biography of Chimalpahin and a detailed account of his writings. The Nahuatl text and translation are presented in columnar parallel arrangement and supplemented by copious footnotes, mostly by Lehmann. Kutscher added a very useful tabulation (pp. 196–223) in chronological order of the events related in the Memorial which shows the scope and importance of this chronicle. Further evaluation and elaboration of this source material, which would also necessitate the revision of certain calendrical correlations, has been purposely postponed until all of the writings of Chimalpahin are reliably translated. Kutscher, who has been active over a period of time in carrying on the work of Walter Lehmann, is to be congratulated for the completion of this handsome volume which makes more easily accessible an invaluable source for the early history of Mexico.

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Teotl and Ixiptlatli: Some Central Conceptions in Ancient Mexican Religion, with a General Introduction on Cult and Myth. Arild Hvidtfeldt. Munksgaard, Copenhagen, 1958. 182 pp. Danish kr. 24.–

The second, specifically Mexican part of this study does not fulfill what the first, general part promises: a treatment of Mexican religious materials as an example of "the rise of the urban religions." In the first part the author accepts Svend Aage Pallis's division of all known religions into those of "gatherers, hunters, cattle breeders and nomads and primitive agriculturists" who "live in direct contact with nature," and "the people of the urban religions" who are "without contact with nature" and where "man himself becomes the only standard of value. . . . These two groups represent forms of religion which are different in kind, whereas all the religions which belong to one or the other of these two groups are specifically related and identical in their fundamental characters." Pallis states that "the gods are the inmost consequence of urban culture." In an earlier publication (the ideas of which our author follows), Pallis sees urban religions foreshadowed in pre-urban agricultural communities: "The agriculturist introduces anthromorphism; the *manas* of the hunting community, each having its own distinct character, each determined by its 'environment,' are conceived as human. Here we meet with a

stage in the development towards the fixed deities of the urban culture. . . ." Hvidtfeeldt believes that the Mexican religious material belongs to some such transitional situation, to "the stage which is characterized by agriculture and incipient urbanization, without, however, it being possible so far to talk about city states of the same type as found in the Near East and the Mediterranean countries in Antiquity." With the Mexican material we are, therefore, "taken right into a community which is found at the stage of which we are most of all in need of getting close information." This is why the comparative student of religion considers the Mexican data as especially important.

A study of the religious materials from ancient Mexico from such an angle is a fascinating program, and besides it is one for which the author seems admirably prepared, because of combining with a broad comparative outlook and knowledge a capacity for minute philological analysis of religious texts which is still rare in the Mexican field. Moreover, as the author correctly stresses (p. 71): "the religio-historical material from the Mexican field is unusually rich. Indeed, considering that the bulk of it originates from the 16th century, it is absolutely unique. Apart from texts belonging to religions which are still alive (we are especially thinking of India), so detailed ritual descriptions have hardly been recorded anywhere in the world until modern times, and even now what has elsewhere been recorded in the way of ritual descriptions in the language of the natives in question is extremely limited." This is the other reason why the Mexican data are so exceptionally important for a comparative study of religion.

Unfortunately the concrete analysis offered in the second part is handled in such a way that it is very difficult indeed to consider it as an exemplification of such a study. We find no end of lovingly presented native text and text analysis, but very little in the way of conclusions drawn from them, unless we are to be satisfied with statements like this: "The material (here presented) freely fits into the previously described general view, and the examples from the Mexican original texts thus may be considered a corroboration of the applicability of the general view," namely, that "certain *mana*-filled objects slowly in the course of time are transformed, in form as well as in function, into idols." The analysis of the native texts is shot through with statements like this: "On a general religio-historical assumption it seems evident that the treatment described is based on concepts of 'mana,'" but it is difficult to find a clear statement as to what the two terms that form the central topic of this book actually stand for. I would say that *teotl* is god as a *concept*, and *ixiptla*, the concrete *appearance* of a god, be this a sacred person or object; but this understanding I arrived at before reading Hvidtfeeldt's analysis, and it may not represent the author's point of view.

If the book as it stands, and more specifically its second, Mexican, part is a disappointment, there is nevertheless no doubt in my mind that a treatment of Mexican religion from a broad comparative viewpoint, such as outlined in the first part, is an urgent necessity, and I feel that the author of this first, not entirely satisfactory, essay may very well be the person to give us such a study.

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Paracas, Nazca, and Tiahuanacoid Cultural Relationships in South Coastal Peru. WILLIAM DUNCAN STRONG. Memoirs of the Society for American Archaeolgy, No. 13, Salt Lake City, 1957. vi + 48 pages, frontis., 18 figs., 4 tables. $1.00.

This paper is an "advance report" on the 1952–53 Columbia University archaeological expedition to the south coast of Peru. It deals primarily with pottery styles and sequence before the Tiahuanaco intrusion, but material evidently is available for a major monograph. This interim study does not include stratigraphic data. Architectural forms have received only preliminary treatment. Many more illustrations are needed to disclose the full range of ceramic styles and decoration. No discussion is given of vegetal remains, weaving, metallurgy, featherwork, or artifacts of wood, stone, bone, and so forth.

In spite of the wealth of archaeological remains in the Ica and Rio Grande de Nazca valleys, very little is known about the conditions under which they are found. Chronological relationships have been worked out by speculative studies of ceramic styles. We hope that the final report of the Columbia expedition will confirm by field stratigraphy many cultural phases which have been based on theory.

An important aspect of the work only briefly touched on in this report is the site survey. A total of 81 archaeological sites are located on a map, and in Table 1 their chronological position is given as revealed by surface collections. Although 46 are recorded as cemeteries, new data are now available for study of the little-known architectural and settlements types of the area.

Nonceramic shell mounds on the coast south of Nazca are described. Presumably these are of great age as no suggestion of agriculture was found. Excavation was almost impossible on account of wind-driven sand. Chipped blades, however, were found, as well as ash, charcoal, shells, bird and animal bones. A radiocarbon date from this material would be welcome. The author points out that other sites of this type, perhaps at Lomas and at the mouth of the Ica river, may be hidden beneath huge refuse beds.

The major portion of the text is a detailed description of ceramic types characteristic of various chronological phases. The present study terminates with the appearance of Tiahuanaco influence in the Nazca area. More recent papers by Menzel and Rowe attack the problem of subsequent coastal phases and styles and discuss Paracas pottery in the light of Dawson's classification.

Strong divides Paracas ceramics into early and late phases. The type site for the former is Juan Pablo; Cahuachi and Ocucaje II are type sites for the latter. There

followed at Cahuachi a hitherto undescribed incised polychrome ware which is named Proto-Nazca. Over this was the Nazca A style of Gayton and Kroeber, which Strong regards as early and middle phases of Nazca. To this point the stratigraphic series is said to be continuous and unbroken. The late Nazca (Nazca B) was not found in direct association with earlier remains. The subsequent Nazca Y phase is called Huaca del Loro.

The classic aspects of the Nazca ceramic series have for many years been designated by the letters A, X, B, and Y. The types are well known through large collections and many publications. Comparatively little Paracas pottery has been published and not much is available for study except in Peru. The illustrations in this study therefore are of importance. The line drawings, published at half scale, are fully adequate but some of the half tones are greatly reduced and lack clarity.

There are 18 radiocarbon dates in Table 4, two from Virú, the others based on material from the south coast. These dates, which were processed by the Lamont Laboratory, seem to be consistent and logically spaced. They are, however, definitely more recent than dates obtained by the University of Chicago from supposedly comparable samples. More tests clearly are needed to establish an acceptable and unquestioned chronology for the south coast of Peru.

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ARCHIVES OF ARCHAEOLOGY

The Society for American Archaeology, in cooperation with the University of Wisconsin Press, has established a new publication series on microcard, entitled *Archives of Archaeology*. The editorial staff consists of David A. Baerreis, Stephen F. Borhegyi, John B. Rinaldo, Thompson Webb, Jr., Director of the University of Wisconsin Press, *ex officio*, and Raymond H. Thompson, Editor of the Society for American Archaeology, *ex officio*.

Microcard publication is not designed to supplant currently used publication media. It is, rather, a supplementary form of publication which makes possible the presentation of primary documentation on archaeological materials and excavations and basic data from collateral fields, such as ethnohistory. Studies that are now abbreviated or highly compressed because of publication costs can now be made available to interested scholars in full. Suggestions regarding format and appropriate material for publication and an evaluation of available microcard readers will appear in the next issue of American Antiquity. In the meantime the editors welcome inquiries and will consider manuscripts for immediate publication.

The first two publications are now available. They may be ordered from the University of Wisconsin Press, 430 Sterling Court, Madison 6, Wisconsin. Remittance should accompany order.

Archives of Archaeology currently available:

1. *A Report on a Bluff Shelter in Northeastern Oklahoma (Dl–47)*. David A. Baerreis and Joan E. Freeman. 334 pages on 5 double-sided cards. $1.50.
2. *An Archaeological Report on a Cave Deposit (Dl–30) in Northeastern Oklahoma*. Joan E. Freeman. 104 pages on 2 double-sided cards. $0.75.

NOTES AND NEWS

EDITED BY NATHALIE F. S. WOODBURY

THE NATIONAL SCIENCE FOUNDATION AND ARCHAEOLOGICAL RESEARCH

The National Science Foundation was established in 1950 as an independent executive agency of the Federal government. Social science was not included in the enumeration of sciences to be supported in the basic National Science Foundation Act (Public Law 507, 81st Congress), but the phrase "and other sciences" following the enumeration permitted the Foundation to develop programs in the social sciences. After a period of study a limited program of support was approved in August, 1954, and in August, 1957, a unified Social Science Research Program was established. In 1958, the Office of Social Sciences superseded the earlier Program, and in 1959 a Program Director for Anthropology and the History and Philosophy of Science was appointed. A change in policy accompanied the creation of the unified Program in 1957; prior to this time the criterion of convergence with the natural sciences was employed in evaluating proposals for research support, but this criterion was then abandoned and the present policy of considering social science proposals without relation to other disciplines was established. The particular business of the Office of Social Sciences is support of basic research and related activities, including conferences and travel to certain kinds of international meetings. Other divisions of the Foundation are concerned with graduate and faculty fellowships, emergency support of scientific publications, translation of foreign science information, and other activities of interest to archaeologists.

The earliest grants in archaeology appear in the fiscal year 1954 (ending June 30, 1954) under the category "General." In 1955, three archaeological grants were made, now under the title "Anthropological and Related Sciences." Subsequent development is set out in the accompanying table:

GRANTS IN ARCHAEOLOGY, 1954–59

Fiscal Year	*Number*	*Amount*
1954	2	$ 35,000
1955	3	30,600
1956	4	50,800
1957	7	63,900
1958	8	102,000
1959	12	219,100
Total	36	$501,400

The steady increase in the funds granted in support of archaeology is obvious, but in spite of this favorable situation demand has always exceeeded supply. In both 1958 and 1959, the total amount granted was about 30% of of the sum requested, and the discrepancy was adjusted by declining proposals and by granting smaller sums than were requested when a reduction was possible. The larger 1960 budget of the Office of Social Sciences will probably result in a higher level of support throughout the social sciences.

Grants awarded in 1959 are: Culture History of the Western Great Basin (R. F. Heizer, University of California, Berkeley), $5,500; Archaeological Exploration of a Maya Site (G. R. Willey, Harvard University), $32,700; Heiroglyphic Writings of the Ancient Maya (W. F. Friedman and E. S. Friedman, Tulane University), $4,000; Prehistoric Man and His Environment in Shanidar Valley (R. S. Solecki, Smithsonian Institution), $23,500; Archaeological Reconnaissance on Okinawa (H. L. Shapiro, American Museum of Natural History), $2,800; An Obsidian Dating Method Applied to California Archaeology (B. A. Gerow, Stanford University), $8,800; Preceramic Cultural Horizons of Minnesota (E. Johnson, University of Minnesota), $9,800; An Archaeological Study of Twin Falls Site No. 1 (A. W. Bowers, University of Idaho), $2,000; Migration Route of Niagara Frontier Indian Villages (M. E. White, University of Buffalo), $4,300; Paleoecological Aspects of Food-production in Southwestern Asia (R. J. Braidwood, University of Chicago), $47,700; Beach-ridge Dating in Alaska (J. L. Giddings, Brown University), $37,300; Interrelationships of New World Cultures (G. F. Ekholm, Institute of Andean Research, Inc.), $40,700.

In addition to strictly archaeological research, the Foundation has aided several projects of archaeological interest, for example, a study of the racial history of the Peruvian coast, preparation of the Handbook of Middle American Indians, and translations of Aztec documents.

The general nature of Foundation policy is well illustrated by the 1959 list of grants. Within the framework of basic research, there are no restrictions on topic or locale. This flexibility extends to interdisciplinary projects, not only within anthropology but also to research involving the collaboration of biological and physical scientists. There are no upper or lower limits on the size of grants considered other than the practical restrictions imposed by the total budget, and grants may be made for periods up to five years. Applications should normally be submitted through the institution with which the principal investigator is affiliated. There are two closing dates each year, February 1 and October 1, and processing takes approximately four months. Prospective applicants are invited to discuss their proposals with the Foundation before formal submission.

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National Science Foundation

An exhibit, "Indian Art of the Americas," was held at the Chicago Natural History Museum, August and September, 1959, as a part of the Festival of the Americas

on the occasion of the Third Pan American Games. A 64-page catalogue of the exhibit with an introduction on "The Diversity of Indian Art" was prepared by Donald Collier.

NORTHWEST

Idaho. Under a grant from the American Philosophical Society to Earl H. Swanson, the Idaho State College Museum carried out a survey of southwestern Idaho during the summer of 1959. The objective of the project was to determine if a boundary zone between Plateau and Great Basin cultures could be defined in that area. The field survey was carried out by Robson Bonnichsen and Gary Baty. The Museum also cooperated with the Peabody Museum of Harvard University in sponsoring excavations at Wilson Butte Cave, near Shoshone. The expedition was directed by Ruth Gruhn, Radcliffe College, who is using the results as the basis for a doctoral dissertation. Work was also continued on the study of prehistoric environments in the Snake River basin under a Sigma Xi grant.

Washington. Crews from Washington State University, under the direction of Richard D. Daugherty, excavated a number of sites in the Ice Harbor Reservoir area, the Rocky Reach Reservoir area, and in the Sun Lakes State Park.

Excavations in the Ice Harbor area, conducted under contract with the National Park Service, were carried out by a crew of 19 students enrolled in the annual summer field school. A number of protohistoric and early historic burials, plus four semi-subterranean houses were excavated on an island in the Snake River. Excavations were also conducted at a housepit village and at several caves which contained cultural materials underlying a thick layer of volcanic ash of Altithermal age.

The excavations in the Rocky Reach Reservoir area were carried out under a contract with the Chelan County Public Utility District No. 1, arranged by the National Park Service. Six camp sites and one rock shelter were excavated. The rock shelter, though small, was extremely rich in cultural materials and gives promise of producing the first good stratified record for the area.

The excavations in the Sun Lakes area, conducted under contract with the Washington State Parks and Recreation Commission, continued the work, begun last summer under the direction of Douglas Osborne. In addition to the scientific value of the work, it is hoped that the excavations will uncover sites that can be restored and thereby expand the interpretative programs in the Park.

British Columbia. A field party, directed by Charles E. Borden, the University of British Columbia, carried out excavations at site DjRi 3 in the Fraser Canyon. This deeply stratified site has an early occupation horizon radiocarbon-dated at 6193 B.C. ± 310 years. Three subsequent cultural levels, separated by massive gravel deposits, have been distinguished. The uppermost horizon is exceptionally rich. Contrary to earlier assumption, the cultural remains in this horizon are probably not characteristic of the Upper Stalo of recent centuries, although, to be sure, the late archaeological manifestations of these local Indians are as yet little known. At any rate, despite marked differences, the closest affinity of the uppermost occupation level of site DjRi 3 appears to be with the Marpole phase at the mouth of the river, which has a radiocarbon date of 950 B.C. Comparative studies, supported by additional dates should shed light on early cultural exchange between upriver and downriver groups.

At the time of writing, some 290 square feet of the 8150 year old horizon had been exposed, but excavation of this level was just beginning. A test pit, a short distance from the main trench, yielded some evidence hinting at an even earlier occupation some 5 feet below the dated horizon. A charcoal sample, but no indubitable cultural material, has been obtained to date from this deeper stratum.

The Fraser Canyon excavations are supported by the Leon and Thea Koerner Foundation, the National Museum of Canada, and the University of British Columbia Committee on Research.

Assembled by Charles E. Borden

CALIFORNIA

A group of anthropologists in Sacramento "concerned with the fostering, promoting and improving of archeological research" has organized the Central California Archeological Foundation. It became an incorporated nonprofit organization on April 29, 1959. The purpose of the Foundation is "to obtain and administer funds for the furtherance of archeological research which has a direct relationship to archeological problems of the Sacramento, California, area." The Foundation members felt that it was important to organize to protect and investigate the archaeological resources in their area. Public attention must be brought to the fact that these resources are fast disappearing in the wake of a population explosion which may ultimately destroy most of the important archaeological sites in this region. Time is short and the public must be aroused to support a long range and comprehensive archaeological program for central California.

William H. Olsen is president of the C.C.A.F., and the executive board is composed of William J. Beeson (Chairman), Brigham A. Arnold, and Francis A. Riddell. There are at present 12 members of the C.C.A.F. including those named above.

The Foundation has received a contract from the State Division of Beaches and Parks for archaeological research at Sutter's Fort State Historical Monument, Sacramento. In addition, the Foundation has received a contract from the same office for an archaeological survey of Fort Humboldt, Eureka. The former project was under the direction of William H. Olsen with the assistance of Louis A. Payen, Archaeologist Foreman, and David S. Boloyan and Philip R. Coleman. The results of this historical site archaeology were gratifying as they strongly tend to support a recently discovered plan of Sutter's Fort made from an original by Sutter. The plan was drawn by Heinrich

Künzel and published in 1848 in an obscure German publication. The foundation of an original corral wall was discovered in the position indicated by Künzel's map, thus verifying the accuracy of his plan. Unexpectedly discovered with a filled pit inside the corral beside the wall. Although not as yet fully excavated the fill seeems to be of garbage of the post-Sutter period. The pit itself, however, may date from the Sutter period and probably served as a cistern or well. It fell into disuse and became a place to throw garbage. Large quantities of cut bone, broken glass, porcelain, and metal eating utensils suggest refuse from a boarding house. The Central Building at the Fort is known to have been used as a "hotel" after Sutter sold out. The refuse from the pit probably stems from this period.

The project at Fort Humboldt was under the supervision of Donald P. Jewell, faculty member of the American River Junior College. He was assisted by John S. Clemmer, Anthony Ishisaka, and Ric Windmiller. Plowing and discing of the fort area by local farmers have obliterated any evidence of the log foundations of the various buildings at this former army post. However, evidence in the form of various subsurface pits such as post holes, privy excavations, and cellars will be an aid in the study of the old fort.

Assembled by FRANCIS A. RIDDELL

SOUTHWEST

PECOS CONFERENCE. The 1959 Pecos Conference was held at Taos, New Mexico, from August 28 to 30. The Fort Burgwin Research Center, an affiliate of the Wichita Foundation, Inc., was host institution. Fred Wendorf served as chairman, and the program was arranged with the assistance of Richard B. Woodbury and Robert H. Lister. Special sessions were devoted to Southwest-Plains relationships and Southwest-Mexico relationships. High point of the meeting was a presentation made by J. O. Brew to James Allen Lancaster on behalf of Southwestern archaeologists in appreciation of Lancaster's 35 years of work in Anasazi archaeology. Registering at the meeting were 160 interested southwesternists, including several from Midwestern and Eastern states. The invitation of the Museum of Northern Arizona was accepted to hold the 1960 meeting in Flagstaff, Arizona; Edward B. Danson was elected chairman of the interim committee.

WETHERILL MESA PROJECT. Under the direction of Douglas Osborne the Wetherill Mesa Archeological Project at Mesa Verde National Park is beginning its second year of a five-year program which will include the following: (1) an intensive survey of Wetherill Mesa; (2) the excavation and stabilization of three large cliff dwellings (Long House, Step House, and Mug House); (3) excavation and stabilization of several smaller surface ruins; (4) excavation outside the park area in sites necessary to the Park Service Interpretive Program; (5) ecological, paleontological, and geochronological studies in conjunction with all of the archaeological projects. A laboratory building has been erected and soon will be in operation. Alder C. Hayes has recorded nearly 400 sites in the survey of Wetherill Mesa, although work thus far has been limited to the sides of the mesa and only a small portion of the 8 square miles on the top. At Long House over 200 rooms and eight kivas have been excavated under the supervision of George S. Cattanach, and Arthur H. Rohn. Work at Long House will be continued, while excavation at Mug House will be started this spring. At least two more archaeoolgists, crew leaders or assistants, will be added.

UPPER COLORADO RIVER STORAGE BASIN. The University of Utah, the Museum of Northern Arizona, and the Museum of New Mexico have all had crews working during a large portion of the past year. The University of Utah and the Museum of Northern Arizona have recently released publications dealing with the first phases of their work.

The Glen Canyon Dam Section. Last summer the Museum of Northern Arizona crew under the supervision of William Y. Adams, and including Alexander J. Lindsay and Christy G. Turner, excavated or tested 13 sites along the river north and northwest of Navajo Mountain. These were occupied for the most part in the 12th and 13th centuries. An especially interesting feature of the work was Lindsay's study of an extensive and elaborate prehistoric ditch system on the south bank of the San Juan River near the mouth of Beaver Creek. The irrigation complex apparently included major diversion ditches, soil terraces, and several series of secondary ditches which carried water from neighboring terraced slopes in evenly graduated descents. Remaining traces of ditch and terrace structures suggest that the original areas under cultivation may have amounted to several hundred acres. Scattered along the edges of the terraced areas are a dozen small habitation sites of late Pueblo II or early Pueblo III age.

The Navajo Dam Section. Crews of the Museum of New Mexico under the supervision of Alfred E. Dittert, Jr., have now located over 500 sites in the project area. Among these is a group of "cobblestone ring sites," possibly of an early Basketmaker period. Tests in these sites have not yielded pottery, but they have shown that the rings outline structures with subfloor bins, an excavated floor, a bench, and worked sandstone slabs on the floor. Diameters of the structures vary from 25 feet to 40 feet with the cobblestone outlines from 3 feet to 6 feet wide. These stone ring sites form part of a typological series which ranges from simple circles to Rosa phase pit houses surrounded by cobblestone rings. Work has been completed on a large Rosa phase pit-house-and-surface-room unit, and stratigraphic tests have been made in a large Rosa phase village preparatory to complete excavation. A Navajo forked hogan site occupied about A.D. 1725 has also been cleared.

ARIZONA. The Museum of Northern Arizona conducted two excavations on the Navajo Indian Reservation last summer, at the request of the Navajo tribe. One of these was a Basketmaker burial containing eight individu-

als. The other consisted of two pit houses and a small two-room pueblo that were to be destroyed by the new Leupp road. This work was done by Paul V. Long, Jr., serving as acting Curator of Anthropology between the resignation of David A. Breternitz and the appointment of Alan P. Olson to this position.

The Museum of Northern Arizona sponsored the Second Southwestern Seminar which was held in Flagstaff, September 19. At the suggestion of Paul S. Martin the White Mountain and Little Colorado redwares were discussed. The conclusions reached will be incorporated in the Museum of Northern Arizona Ceramic Series.

During the 1959 season, the expedition of the Chicago Natural History Museum, under the direction of Paul S. Martin and John B. Rinaldo, undertook three projects: a survey of the area between Show Low, Snowflake, St. Johns, and Springerville; the excavation of two surface sites; and the excavation of a cave site. The survey, by William Longacre of the University of Chicago, recorded over 100 sites ranging from prepottery times to late Zuni glaze times. A small site, started last year and yielding mostly Snowflake Black-on-white, was finished this season. Excavation was begun in a large site on the Little Colorado River, on the Hooper ranch just west of Springerville. Two dozen rooms and a kiva were excavated.

The University of Arizona Archaeological Field School concluded its 14th consecutive season at Point of Pines on August 7, 1959. Seventeen students, representing eleven institutions and Sweden, Italy, Tonga, and Canada, in addition to the United States, participated in the work. Operating under a grant from the National Science Foundation which permitted the employment of an Apache work crew of fifteen, the school concentrated on two main problems: (1) The excavation of a Great Kiva of Reserve phase age in a ruin on the Dry Prong tributary of Eagle Creek 25 miles from the field school headquarters. This work, directed by Alan P. Olson, provided a needed link in the developmental picture of the Great Kiva as an architectural form in the Point of Pines region — spanning about 1000 years. (2) Conclusion of the work in the Turkey Creek Pueblo, of late Reserve–early Tularosa phase times, which was begun last year. About 95% (306 rooms) of this community has now been cleared and the accumulated information, added to that from extensive trash mound and burial excavation, provides an exceptionally complete sample for a village of this size and age. Information was obtained on the time of appearance of certain pottery types, such as Querino and Houck polychromes and San Carlos Red-on-brown, in the local pottery sequence. A question of major importance raised by the work concerns the wholesale importation of black-on-white pottery from production centers north of the White Mountains, a practice generally not attributed to the ancient Southwestern potters. Alfred E. Johnson served as foreman on the Turkey Creek operation, aided by Olson upon the conclusion of the Dry Prong excavation, and Wilma Kaemleim, of the Arizona State Museum staff, was in charge of the field laboratory. The over-all program was directed by Emil W. Haury assisted by Raymond H. Thompson.

At the beginning of last summer the Arizona State Museum was commissioned to perform salvage archaeology in a number of ruins on the Navajo Indian Reservation in the vicinity of Lupton in connection with a new alignment of Highway 66. William W. Wasley was in charge of the project, which lasted one month, with David A. Breternitz and Alfred E. Johnson as assistants. Excavated during this time were 16 pit houses, 43 surface structures, five kivas, four trash mounds, 20 burials, and 22 miscellaneous and isolated architectural features. Two other kivas were outlined but were not completely excavated. This material ranges in time from Basketmaker III to Pueblo III, although Pueblo I was not represented at all in this material and Pueblo III was represented only by a single isolated farm house. Of particular interest was a new type of Basketmaker III site with very large circular pit houses and both gray and brown pottery, and a White Mound phase site which contained no Pueblo I admixture, so that it provides a clearer picture of this phase than was possible at the White Mound site excavated by Gila Pueblo.

Working on weekends and through the summer of 1958 and part of the 1959 summer, Bernard L. Fontana and William J. Robinson, assisted by other graduate students in the Department of Anthropology at the University of Arizona, have excavated architectural remains adjacent to the Franciscan structure of Mission San Xavier del Bac. They also ripped out a concrete floor in one of the rooms of the modern church to test for levels of occupation. The present structure, which was built between 1783 and 1796, rests on a small basaltic hill. The accumulation of soil on this hill and of cultural deposits ranges from a scant 5 cm. to 1.5 m. in depth. To the west of the church the workers uncovered the remains of an atrium wall, a rectangular enclosure that was approximately 23 by 7 m. Inside and immediately adjacent to the wall was a walkway of sun-dried adobe bricks. The enclosure contained the floors and foundations for three small storage rooms, a patio, and blacksmith shop, and a single row of arches whose circular foundations remain. A large structure with stone footwalls nearly a meter wide running south of the mission was partially excavated. The work was stopped when the ruins intersected a county road. Further testing on all sides of the church have contributed significantly to the knowledge of this late 18th century mission, various other room foundations and an old brick drain having been located. Analysis of the historical artifacts, including remains of copper and iron, non-Indian ceramics, textiles, and Pima and Papago Indian wares is proceeding. When the results of the excavations are analyzed and combined with the results of a continuing historical research program into archival materials, it will perhaps be possible to draw a detailed picture of life at Franciscan San Xavier and in Franciscan Pimería Alta.

The Ruins Stabilization Unit of the Southwest Archeological Center at Globe, Arizona, a field organization under the Region Three Office, Santa Fe, completed another active season involving the repair and maintenance stabilization of sites in various Southwestern

National Monuments including Tonto, Tumacacori, Chaco Canyon, and Aztec.

Plans and arrangements were made for the construction sometime in November 1959, of a protective shelter for the Sacaton phase pit house which was excavated late in 1958 near Montezuma Well by the Museum of Northern Arizona in cooperation with the National Park Service. The shelter is designed to achieve two things: (1) preserve in place the floor and features of a Hohokam type communal dwelling, and (2) permit its use as an interpretive exhibit for the benefit of the visiting public.

Gordon Vivian recently completed a manuscript which is to be published by the National Park Service as one of a series of research studies devoted to specialized topics which have been explored in connection with various areas in the National Park Service. This report deals with the excavation of the Hubbard Mound at Aztec National Monument, and other tri-wall structures in Colorado and New Mexico. It brings together for the first time a full discussion, comparisons, and a new hypothesis concerning these bizarre structures which occupy a rather limited areal distribution within the Anasazi cultural province.

During stabilization in July and August of the ruin known as Talus Unit in Chaco Canyon, Joel Shiner conducted a deep stratigraphic test in a Chaco type kiva containing Mesa Verde trash. He found that the low horizontal masonry pilasters on the bench supported in situ a series of cribbed logs of the former roof. Treering dates have not yet been obtained from the saved beam specimens, but it is hoped construction date of the kiva will eventually be derived. A preliminary analysis of the sherds from the refuse indicates a date of about A.D. 1200 for the final and non-ceremonial use of the kiva as a trash repository. Some new light may be shed concerning the Mesa Verde reoccupation of a major Chaco site.

Roland Richert recently completed his report of excavations incidental to stabilization of 13 rooms of the East Ruin, Aztec National Monument. On the basis of architectural and artifactual evidence, this small sector of a largely unexcavated site indicates construction and occupation by a "pure" Mesa Verde branch group from about 1225 to 1300.

CHIHUAHUA. Under the direction of Charles C. Di Peso, the Joint Casas Grandes Expedition of the Amerind Foundation, and the Instituto Nacional de Antropología e Historia began its second year of field operation on September 1, 1959. During the first year 12 individual architectural units were exposed at the main Casas Grandes site. These included a large "I" shaped ball court with an attached truncated platform, four puddled adobe secular living units, and a series of ceremonial mounds. An architectural sequence is being worked out and information derived concerning the use of interior steps, complete with landing and bannister, and the use in secular units of windows and of large stone discs as capitals and bases of pillars. Evidence of human sacrifice has been encountered.

COLORADO. The University of Colorado Museum, represented by Joe Ben Wheat, continued work near Yellow Jacket last summer. Excavation in a Pueblo II–Pueblo III site revealed a series of pit rooms built in a circle around a kiva which was entered by a tunnel from one of the pit rooms. Surface rooms were post and adobe structures. The evidence at this site presented by skeletons of four individuals ranging in age from about seven or eight years to about 19 years points definitely to cannibalism, the second recorded instance of this practice in the Southwest.

NEW MEXICO. The National Park Service undertook a test excavation in a rock shelter at Carlsbad Caverns National Park in January, 1959. The work was done by Archeologist Z. A. Bradley and Park Naturalist Paul Spangle and revealed an Archaic "Desert Culture" horizon in the lower level of a 6-foot deep test trench.

Dedication ceremonies for the new visitor's center at Chaco Canyon National Monument were held last August. Neil M. Judd, Associate in Anthropology, U.S. National Museum, represented the National Geographic Society which sponsored his research at Pueblo Bonito.

The summer field school of the University of New Mexico, with 42 students and five student-assistants under the direction of Florence Hawley Ellis, spent six weeks excavating in the old site of Yunque, or San Gabriel, the first Spanish capital of New Mexico. Oñate, arriving with 125 colonists and their families and servants, came to San Juan Pueblo in 1598 and settled there for some months.

When another 80 colonists and their families arrived, the entire settlement was moved to the west side of the Rio Grande and occupied a pueblo said to have been of about 400 rooms, from which the Indians moved. Last summer's excavations indicated that the spot onto which the Spaniards moved was the west wing of a horseshoe-shaped pueblo, partly in ruins at the time. The colonists leveled the old pueblo by cutting off the upper parts of walls and filling the vacant rooms. Upon this they built their village of contiguous rooms, Indian style except for the use of typical Spanish stone footings and some use of doors between rooms. A scattering of metal and other Spanish-imported objects was found, as well as Indian pottery, in the Spanish levels; this was not the case in the fill below. The Spaniards moved their capital to Santa Fe in 1610. The east mound of the old pueblo appears to have been occupied only by Indians, but contemporaneously with the Spanish occupation of the west mound. Approximately 150 rooms were excavated at Yunque.

The Museum of New Mexico conducted excavations in two mission structures under the supervision of Stanley Stubbs. During the course of stabilization work at Quarai Mission an earlier mission structure was uncovered. Located in the main plaza of Quarai Pueblo just to the southwest of the main mission, this building probably represents the first Spanish construction at the pueblo, and it served as the mission church until the main church and convento were completed. The shape is noncruciform with a simple tapered sanctuary. No datable mate-

rial was obtained, but the building date should be around 1615–1620. A Spanish mission long rumored as existing, and referred to in the archives as "a mission to the east of Gran Quivira," was partially excavated. The building is completely enclosed in the Plaza of Pueblo Blanco (LA 51), about 15 miles northeast of Gran Quivira. This evidence probably locates and identifies the site as that of Tabira, one of the early missions in the Jumanos area of central New Mexico. Like the early structure at Quarai, this building has the simple tapered sanctuary, but it is much better preserved.

TEXAS. Rex Gerald of the Centennial Museum, Texas Western College, El Paso, surveyed five small flood control dam sites on Alamo and Diablo arroyos near Ft. Hancock, Texas, in the spring of 1959, and located nine sites. The work was done in cooperation with the National Park Service. In June, three of the sites were tested but almost nothing was found below the surface. They appear to have been seasonally occupied during the Dona Ana phase.

Also in June, Gerald excavated a circular pit house, partially destroyed by erosion, at the El Paso Electric Company site in New Mexico across the Rio Grande from El Paso. No sherds were found, but further excavation is contemplated.

Assembled by WILLIAM W. WASLEY

GREAT BASIN

The Great Basin Archaeological Conference, with Phil C. Orr as Chairman, held its sixth annual meeting at the Santa Barbara Museum of Natural History, Santa Barbara, California, September 10–11, 1959. Hosts were the Museum and the Western Spcleological Institute. Approximately 80 persons attended the two-day meeting. The program was organized by Ruth D. Simpson.

Problems of dating archaeological finds were discussed at the opening session with papers as follows: George Kennedy, "Application of Thermoluminescence in Dating"; C. W. Ferguson, "Woody Desert Plants as Dating Tools"; Dick Shutler, Jr., "Recent Trends in Palynology"; Bruno E. Sabels, "Paleoclimatological Studies by Emission Spectography"; and Alex Krieger "Early Man in the Great Basin." Six field reports given during the next session were: William Wallace, "Anza–Borrego Desert State Park Investigations"; Dick Shutler, Jr., "Nevada State Museum Archaeological Program"; William B. Wright, Jr., "Archaeological Surveys in Nevada and Utah"; Marshall McKusick, "A Peripheral Puebloid Site"; Keith L. Johnson, "Antelope Cave Excavations"; and Ben Swartz, "The Klamath County Museum Archaeological Program." Two additional reports pertaining to chronological methods were presented at the third session; Joseph H. Birman discussed "Glacial Advances in the Central Sierra Nevada" and Donald E. Savage described "The Role of Fossil Mammals in Geochronology." The remainder of this session was devoted to pictographs and petroglyhs with reports as follows: Ruth D. Simpson and Willis Grafton, "Big and Little Petroglyph Canyons"; Charles S. La Monk, "Petrography in Southern California"; William C. Miller, "Can Pictographs and Petroglyphs be Dated?"; and Dale W. Ritter, "Massacre Lake Petroglyphs." The 1960 Conference will be held at the University of Oregon; Luther Cressman was elected Chairman.

Assembled by WILLIAM J. WALLACE

PLAINS

TEXAS. W. W. Crook, Jr., and R. K. Harris, of the Dallas Archeological Society, announced in July the determination of a radiocarbon date of 5945 ± 200 B.P. from a late Carrollton focus component in the lower Patillo formation in the Union Terminal–Carrollton terrace (T-1) of the Trinity River at the Wood Pit site. The date was obtained at the Field Research Laboratory of the Magnolia Petroleum Company in Dallas. This 4000 B.C. date accompanies the latest occurrence in the area of paleo-Indian points (as minor types in Archaic sites), net-sinkers, heavy use of gravers, broken projectile points reworked as drills, and Carrollton axes. The associated fauna is entirely modern and includes *Bison bison*.

The Texas Archeological Salvage Project at the University of Texas has had a number of projects under way. In March, L. F. Duffield and W. A. Davis made a preliminary survey of the area to be covered by the enlargement of Lake Waco in Central Texas. Recommendations have been made for the excavation of several sites. In August, E. B. Jelks, Duffield, and a field crew worked for two weeks at the Kyle Rock Shelter in the Whitney Reservoir Area on the Brazos River northwest of Waco. A sequence of projectile points for the post-Archaic Central Texas aspect was recovered from a deposit some 6 feet thick, with Perdiz and Cliffton points above and Scallorn points below. Underneath this deposit were about 3 more feet of cultural deposit with insufficient material to determine whether it is Archaic or post-Archaic. The shelter is unique in Central Texas in that it is relatively dry, so that a few specimens of coiled basketry, arrow shafts, and other perishable items were recovered.

Late in August, J. F. Epstein and J. R. Gipson, assisted by members of the Comstock Archeological Society, spent two weeks excavating at Javelina Cave in the Diablo Reservoir area on the Rio Grande upstream from Del Rio. The material has not been analyzed, but looks different from the usual Pecos River focus material found in the area. At the same time another Salvage Project party made up of Duffield and Johnny Greer made a survey of the Navarro Mills Reservoir area northeast of Waco in Central Texas, finding sites ranging in probable age from paleo-Indian to historic. In September, Curtis D. Tunnell and a field crew spent four weeks working in a stratified rock shelter in the Canyon Reservoir area on the Guadelupe River about 60 miles southwest of Austin. Central Texas and Edwards Plateau materials are represented.

Epstein, in going over the materials from his work at the Diablo Reservoir in the fall of 1958, has found a number of good burins, made on flakes and reworked from scrapers by the standard burin-stroke technique. As a

result of this find others at the University of Texas have been re-examining their collections, and small numbers of burins are turning up, apparently on the paleo-Indian and Archaic levels. Epstein suspects that once people start looking for them, burins will turn out to be scattered well over America. At present the nearest recorded find is in Oregon, and next beyond that the Arctic.

H. F. Alexander, Jr., graduate student at the University of Texas, is excavating the Levy Rock Shelter just west of Austin, and is finding classic Angostura points without any admixture of Archaic materials. This is the first good paleo-Indian component in this area.

KANSAS. A Kansas State Historical Society–National Park Service field party led by Roscoe Wilmeth, with Curtis D. Tunnell as assistant, worked from July 1 to August 21 in the Pomona Reservoir area on 110 Mile Creek, a tributary of the Marais des Cygnes River west of Ottawa in eastern Kansas. Most of the time was spent at the Hart site, 14OS305, a village site, where an oval house was excavated. It had six central support posts arranged in two rows. There was a trash pit in the center, and no prepared fireplace. It appears to have been a wattle-and-daub structure rather than an earth lodge. The pottery is cord impressed, without collared rims, and with no apparent temper. Arrow points are small and triangular with side and base notches. The house and some pottery rim forms suggest relationships to the southeast, though the pottery is basically Central Plains in type. In site 14OS301 a stone-lined grave had been looted by pot hunters. In the back dirt of the looting several hundred shell beads and a few copper ornaments were found. The grave probably dates from the Sac and Fox occupation of 1846–68.

NEBRASKA. The Anthropology Field School of the University of Nebraska, under Crew Chief Tom Witty, spent two weeks in June checking new exposures created by construction work at the Sterns Creek (Walker Gilmore) site in the Missouri valley east of Lincoln. This is an important buried Woodland site investigated several times in the past 50 years by Sterns, Strong, Champe, and others. It also contains materials older and younger than Woodland. In July the Field School spent three weeks at the Lynch site on Ponca Creek in northeastern Nebraska, excavating three earth lodges and associated cache pits. This site was trenched by the W.P.A. in 1936 and claims of great antiquity were made in the press because of the several feet of loess which overlay the cultural zone. The artifacts, however, seemed characteristic of the earth-lodge peoples of Middle Ceramic time and suggested affiliations both with the Middle Missouri to the north and the Central Plains to the south. The work last summer proved, as expected, that the materials recovered in 1936 were indeed associated with earth lodges.

A field party of the Nebraska State Historical Society under Marvin F. Kivett, with David M. Gradwohl as assistant, worked from June 9 to August 1 at the Logan Creek site, 25BT3, in Burt County 80 miles north of Lincoln in east central Nebraska. This was the second season, the first work having taken place in 1957. The site is now known to have four lithic levels, representing a single complex, with a Woodland occupation present in the overlying humus zone. The points in the lithic zones have wide shallow notches and often have basal grinding. The second zone from the top has a radiocarbon date of 6633 B.P.

Kivett also spent part of the summer on highway salvage work. He did two days' work at the Wright site, 25NC3, near Genoa, where a highway borrow pit was cutting into a Lower Loup (protohistoric Pawnee) village, into which Pawnee burials of the 1860–75 Reservation period had been placed.

In northwestern Nebraska Roger T. Grange, Jr., of the Ft. Robinson Museum of the State Historical Society, has been doing highway salvage surveys — mainly with negative results — as well as conducting historic archaeological research and stabilization at the Red Cloud Agency site near Crawford.

SOUTH DAKOTA. The state highway salvage program in South Dakota has progressed to the field work stage. William Buckle is doing survey work for the University of South Dakota Museum, and one large Woodland mound site is already slated for excavation with highway salvage money.

The principal work in South Dakota in 1959 was, as in previous years, salvage work in the two reservoirs still under construction on the Middle Missouri — Oahe and Big Bend. The Missouri Basin Project of the Smithsonian Institution, operating on a much-reduced budget compared to previous years despite the continued salvage emergency, had four crews in the field. Warren W. Caldwell and a crew of six men, during a nine-week period from June 4 to August 6, sampled 19 prehistoric sites in or near the construction area of the Big Bend Dam in Lyman County. The sites were found to represent a series of preceramic camp sites; a group of Middle Woodland burial mounds; and nine small villages of the earth-lodge, sedentary farmer cultures. Charles H. McNutt and a crew of 12 men, during the eight-week period from July 1 to August 28, conducted sample excavations in 22 prehistoric sites in the Little Bend area of the Oahe Reservoir in Sully County, some 40 miles northwest of Pierre. Two of the sites were small rock cairns that yielded very little information. The rest were large earth-lodge village sites of circular houses, representing the La Roche and Stanley periods of sedentary farmer cultures, ranging in age from the 16th through the 18th centuries.

Alfred W. Bowers and a Smithsonian crew of five men, during the seven-weeks period from July 13 to August 28, completed intensive excavations, begun in 1957 and continued in 1958, at the Rygh site (39CA4) near Mobridge, in the Oahe Reservoir area. At least four successive occupations have been differentiated at this site. The earliest was a village of rectangular houses with a fortification palisade and dry moat surrounding it. This was succeeded by three later villages of circular houses, two of which were also fortified with palisade and moat. The time range of these four occupations extends from perhaps as early as the 8th century to the late 18th century of the Christian era.

William M. Bass, physical anthropologist with the Missouri Basin Project, assisted in excavations of the burials at several of the Smithsonian digs just mentioned, and with one assistant visited repositories of skeletal materials at 17 locations in the Missouri Basin area. He recorded anthropometric data on the skeletal remains of approximately 2500 individuals, most of the material being well preserved and adequately documented. Bass is making a comprehensive study of Plains physical types through more than 1500 years.

Carlyle S. Smith and a University of Kansas–National Park Service field crew excavated one earth lodge and several test pits in a large village site in the Big Bend Reservoir.

In August there was a conference of Middle Missouri archaeologists at Pierre. Such meetings are assuming the character of a Summer Plains Conference; this one was dubbed the Third Accidental, or 16½th, Plains Conference.

North Dakota. In the North Dakota part of the Oahe Reservoir area, James H. Howard and a field party sponsored by the University of North Dakota, the State Historical Society of North Dakota, and the National Park Service, excavated two rectangular houses and a bastion at the fortified Huff site.

In western North Dakota, a University of Montana–National Park Service field party under Dee C. Taylor excavated historic structures at Theodore Roosevelt National Monument.

Wyoming. The Wyoming Archaeological Society, previously active mainly around Sheridan, formed a new chapter in Casper in March, 1959, which soon built up to 55 members. As their first work they undertook excavations, supervised by experienced members from Sheridan, in the Hole-in-the-Wall country of the Big Horns about 70 miles airline northwest of Casper. From June 20 to 29 two rock shelters in site 48JO303 were dug. One was nicely stratified in three zones. The middle zone had McKean points, and the bottom zone had Meserve points. Raymond Bentzen of Sheridan ran this dig, assisted by from four to 25 people depending on the day of the week. From July 4 to 18 Glen Sweem supervised work in another rock shelter, 41JO301, which also was stratified and contained a number of pictographs and petroglyphs. It is encouraging to note that Casper, formerly a major center for the buying and selling of paleo-Indian points looted from sites, now begins to be a center for the discovery of, rather than the destruction of, culture history.

George Agogino of the University of Wyoming has recently collected charcoal samples for radiocarbon dating from the Lindenmeier and Agate Basin sites. A good date from Lindenmeier should provide a long-awaited check on the Folsom point date of 9883 B.P. obtained some years ago at Lubbock, Texas.

Montana. Thomas F. Kehoe of the Museum of the Plains Indian, assisted by Sara P. Watrous, worked for a second and final season at the Boarding School Drive site, 24GL302, on the Blackfeet Reservation north of Browning, from mid-June to late August. Alice B. Kehoe and Esther Fishman ran the laboratory, where analysis has been going on ever since the end of the 1958 field season. The work has been supported by the Louis W. and Maud Hill Family Foundation. At the site, large sections of kills were exposed, including bison 12 feet below the surface, estimated as 4000 years old by Earl H. Swanson of Idaho State College Museum who is investigating the geology of the site. The main drives probably took place during the autumn or early winter. The bison in the upper 8 feet of deposit are modern; those below have not yet been analyzed. The site has revealed methods of killing, butchering, and utilizing bison of prehorse Northwestern Plains Indians, and will also furnish information on the ecological history of the area.

Alberta. Richard G. Forbis of the Glenbow Foundation in Calgary, assisted by Tyler Bastian and Donald R. King, spent the summer of 1959 at a number of sites in the southwestern part of the province. At the Old Woman's Buffalo Jump a second and final season of work has demonstrated temporal changes in projectile point frequencies. The sequence at this site should help date the many collections in the area. In July and August, tests were made at a stratified campsite adjacent to a bison jump near the confluence of the Highwood and Bow rivers. Disappointingly few specimens were recovered. Plans for the late summer included work at protohistoric sites in the high Rockies and testing of a probable paleo-Indian site on the Oldman River near Taber.

Saskatchewan. From June to August two field parties of the Canadian National Museum and the Saskatchewan Museum of Natural History, led by W. J. Mayer-Oakes and Z. S. Pohorecky respectively, conducted a survey in the area of the proposed South Saskatchewan River Reservoir in the southwestern part of the province. Two sites threatened by immediate destruction were tested, but the major emphasis was on reconnaissance and more than 135 sites were recorded. At least one more season of intensive reconnaissance is needed to cover the entire 125-mile length of the reservoir area. The sites appear to represent a time span of 4000 to 5000 years.

Assembled by E. Mott Davis

CORRECTION

In the article *Ceramic Profiles in the Western Mound at Awatovi, Northeastern Arizona*, by Robert F. Burgh, published in Volume 25, Number 2, pages 184–202, the sixth sentence under the section, "Stratigraphic Variations in Different Rooms," on page 196 should read as follows:

If none of the adjoining rooms had been excavated, the conclusion would have been drawn that orange-paste polychromes, contemporaneous with black-on-white, were being made when these rooms were first occupied, and furthermore that Jeddito Black-on-orange pottery was not made or used until some years or decades later.

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